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Reminiscences about Many Pitfalls and Some Successes of QFT Within the Last Three Decades

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1 Introductory Remarks

Laymen and sometimes even physicists think of natural sciences, in particular of theoretical and mathematical physics often as subjects, which unfold according to an intrinsic logical pattern, with the limitations being set only by the conceptual and (in case of mathematical physics) mathematical developments of the times. This view certainly cannot be maintained in view of the present stagnation and crisis which in particular affects QFT, an area which in the past has been most innovating and fruitful, also in relation to other important areas of theoretical physics.

In this article I try to analyse this situation using my personal experience of 30 years of scientific carrier life. One of course always hopes that by doing this one may recover physically fruitful ideas which either got lost completely or at least to the younger generation. In good times this may be only of historical interest, however in times of stagnation and crisis one may expect that the past may suggest new avenues into the future of new concepts and principles in physics (and not just as a service to produce new mathematics as it seems to be presently). I want to emphasize that in writing these notes, I followed some natural current of recollections (written within three weeks, during a serious health crisis) and I certainly did not attempt to give a complete account of all contribution which had an influence on my work and on my thoughts. So e.g., the analytic work on S-matrix properties (Lehmann, Oehme, Martin,...), the work of Goddard, Kent, Olive as well as Nahm on algebraic conformal QFT and also some recent and not yet fully published work of Longo, Rehren, Roberts and also Wassermann (investigations of the field algebra – observable algebra connection by using Jones subfactor theory) are not mentioned at all. Recently I also became aware of promising attempts of using chiral conformal QFT for semi-phenomenological analysis in condensed matter physics (edge states in layered quantum structures). But since I was unable to relate the interesting work of Affleck, Fradkin, Fröhlich, Laughlin, Ludwig, Wilszek and many others to my idea of universality classes of quasi-particles (see the last section) in any convincing way, also that work will go uncommented. Even contributions with some own involvement as the old investigations of “semi-nonperturbative” (abstraction from every order of renormalized perturbation theory) “soft” versus “hard” symmetry-breaking of Symanzik, Coleman, Stora, Ben Lee, Jackiw, and others or the recent partially successful attempt to understand space-time covariance from raw local quantum physics (using the Tomita-Takesaki modular theory) by Borchers and Wiesbrock will go unmentioned in the text. This would have overburdened these notes and diminished their spontaneity.

Last but not least this would also have created an aura of a “wizzard on everything”. I am certainly not even close to anything like this. My understanding of recent geometric developments and string theory is flimsy, and even in such physically important areas as the GSW semi-phenomenological theory of elec-

trou weak interaction (and its extensions), I do not really feel at home (admiration about achievements is not enough!)

After a very long absence from algebraic QFT, I returned to this area (like the prodigious son in the bible) around 1987, naturally being very impressed by its gains since 1968. In such a spirit of enthusiasm, one is inclined to proselyte others. I was not successful in that, perhaps with one minor exception. In 1988 at the XVII International Conference on Differential Geometric Methods in Theoretical Physics, I met Ivan Cherednik and he seemed to like those ideas. We emptied a bottle of russian vodka together (from which also Ruth Lawrence took a very well-supervised sip).

In these notes, I use the terminology “algebraic QFT” in a bit more general sense than that of “net-theory” (with the exception of the last section). I call “algebraic” everything which is not obtained by functional integrals or perturbation theory. I also take the liberty of not giving references to those papers which are common knowledge in physics or mathematics (thus protecting these notes against any non-intended misuse).

The speed of progress in theoretical physics always has been more determined by discoveries than by inventions. There are two types of theoretical discoveries, those which are directly motivated by and linked to an experimental discovery (often with considerable hindsight and guesswork) and there are also purely conceptual discoveries in which confrontation with known principles and resolutions of apparent paradoxa play the prime role. The latter type of discoveries are often made by physicist with a considerable mathematical physics background i.e. physicist who know to use the art of mathematics for conceptual precision in physics. Certainly the early development of Q.T. as a description of atomic spectra belongs to the first category.

One is also inclined to count the discovery of QED and its renormalization into this category, although the necessity of quantization of the Faraday-Maxwell field as a matter of consistency with the principles of Q.T. of charged particles by Bohr and Rosenfeld has a strong element of a pure theoretical discovery. Most of Einsteins discoveries (special and general relativity) consisted in extracting new principles from already existing formulas or equations. The Kramers-Kronig dispersion relation which dominated particle physics in the later 50^s is another example of a pure theoretical discovery: it was obtained by investigating the consequences of Einstein causality within relativistic scattering theory. The importance of its subsequent experimental verification in forward high-energy nucleon-nucleon scattering cannot be overestimated. Our confidence that causality and locality hold up to present day energies (or short distances) is founded on these verifications. The protagonists of this modest (in its physical aims) but nevertheless important discovery should not be blamed for the fate which these ideas suffered afterwards when more ambitious people tried to convert them into

the first ¹ “TOE” (to be more precise, a theory of everything minus quantum gravity).

It is interesting to observe, that the mechanism according to which “TOE’s” are invented always seem to follow a similar pattern. One formulates formal as well as often physically reasonable requirements (the latter came from QFT) in such a way that one encounters a highly nonlinear and extremely uncontrollable (both in a mathematical and physical intuitive sense) situation. Usually such structures resemble the infinite nonlinear system of the Schwinger-Dyson equation in QFT. Although one has no solution at all, one hopes (or dreams) that it has a unique solution. After many years of thinking one finds that such a “bootstrap” situation has myriads of solutions, usually by “defusing the nonlinear dynamite” via additional physically motivated ideas leading to a linearization.

In the case of the S-matrix bootstrap for two dimensional QFT the key idea which linearized the “nonlinear dynamite” was the factorization equation which characterizes the family of two-dimensional integrable local QFT’s. We will cast some additional light on this interesting (not only historically) situation from a more modern point of view later on.

Two important yet even more modest discoveries of the last decades with experimental verifications (and in the second case even additional consequences) are the Bell inequalities and their maximal violation and the appearance of Berry’s phase in different areas of atomic, nuclear and optical physics.

The issue of QCD and more generally of nonabelian gauge theories in the context of discoveries versus inventions is somewhat complex. On the one hand there is the minimal interaction recipe and the quantization rules by which the classically crystal-clear Maxwell-like theory of fibre bundles is supposed to be converted into a consistent QFT whose intrinsic quantum content remain somewhat hidden (and is only expected to show up after not yet known laborious nonperturbative calculations or by Monte Carlo simulations of lattice systems which are expected to lie in the same critical universality class). This has clearly more the ingredients of an invention. On the other hand the observation that non-abelian gauge theories in the perturbative region are among all local renormalizable Lagrangian field theories the only ones (with some restriction on the number of matter fields) which exhibit the experimentally relevant phenomenon of asymptotic freedom is clearly a purely theoretical discovery. If one could derive it from a renormalization group equation not containing a gauge- and renormalization scheme dependent coupling constant, but rather a physical mass ratio (and therefore may also have an intrinsic meaning in the nonperturbative regime) then the asymptotic freedom statement would even get the status of a structural theorem like the nonperturbative version of the Nambu-Goldstone-theorem or the “free charge versus screening” theorem of abelian gauge theories.

¹The first TOE I met in my life was however not this but (during school-time in the GDR) rather Marxism-Leninism.

Last but not least, the elaboration of the unified perturbative electro-weak theory and its numerous experimental verifications makes it the most successful phenomenological discovery of all times up till now. But to transform this scheme into a fundamental theory with clear principles and fewer parameters has turned out to be difficult indeed. One gets more and more the impression that the prize for that instant phenomenological success was (like in Greek mythology) the entering of a labyrinth from which there is no easy way out.

In fact the theoretical situation has become so complicated that even formally motivated proposals coming from mathematicians (as “non-commutative geometry”) would be accepted gratefully if they only lead to simplifying, parameter-reducing prescriptions (like e.g. the minimal gauge coupling rule).

Inventions which either lack an experimental basis or are not asked for by known theoretical principles (as a string theory of everything, or the more recent formal q -deformations) presently seem to enjoy a large amount of popularity. It is my conviction that this is more related to the sociology of contemporary theoretical physicist and their rapid electronic communications than to the actual physical content of these inventions. To be sure, inventions in earlier times were as copious as they are nowadays. But as a result of a smaller number of protagonists and researchers, and also as a result of lesser sophistication of their mathematical description, they had a greater chance to fade away by natural death in case they did not work in physics.

However, having said this, I do not want to be misunderstood to nourish nostalgia for a mathematical stone age in physics or any restrictions to pedestrian methods.

But sociology in theoretical physics has changed definitely from what it used to be. When Einstein together with just one or two collaborators worked on the aborted unified theory, there was no lack of courage to tell them that they were on the wrong physical track.

2 The Early Days of Algebraic QFT

Already very early in the history of QFT, there was the desire to obtain more insights by involving pure quantum principles without any classical parallelism referred to as “quantization”.² The most successful early attempt was that of Wigner who worked out a complete classification scheme of relativistic one-particle states by purely group theoretical means, [1] i.e. by classifying irreducible representation of the proper orthochronous Poincaré group and later even including the various reflections. The methods were those of “induced representa-

²As Haag pointed out in his book [1], Bohr’s famous remark on the classical versus quantum relation (“we must be able to tell...”) has often been misinterpreted. Bohr’s correspondence principle, that quasi-classical structures appear under very special circumstances, does not constitute a free ride for “quantization” either.

tions” (the “little group”-method) going back to Frobenius and later perfected by Mackey. The method immediately led to local free field equations, and it was very easy to decide when two apparently different-looking system of linear field equations (say obtained by the quantization method) are equivalent. However in spite of Wigner’s work, only in the early fifties the senseless mass production of new higher spin field equations subsided when physicists began to appreciate the significance of Wigner’s work and the relevance of Fock space. This work together with the seminal paper on superselection rules (more specifically the univalence superselection rule between half-integer and integer spin states) by Wick, Wightman and Wigner [1], set the stage for the two important formulations of framework for nonperturbative QFT: Wightman’s theory [2] (with important contributions of Lehmann Symanik, Zimmermann, Glaser and Nishijima) and the Haag-Kastler theory (with important contributions of Araki and Borchers). Whereas the first started by “axiomatizing” physical and technical properties of local covariant fields (with LSZ scattering theory giving the bridge to Wigner’s one particle states and their multiparticle extension by free fields in Fock space), the latter vastly extended the W-W-W-superselection idea to generalized charges and viewed QFT as an operator-representation-theory of nets of observable algebras, thus emphasizing the locality principles known from the classical Faraday-Maxwell theory ab initio (i.e. not through quantization) in an algebraic (von Neumann algebras, C^* -algebras) setting.

“Axiomatic” QFT was, at least at the beginning, just a pragmatic compilation of all those physically motivated properties which, at that time, were susceptible to a reasonably clear mathematical formulation (but without caring too much about the possible interdependences) . The word “axiomatic” attributed to this list an aura of permanence and mathematical and physical stability which was somewhat detrimental. The more recent terminology “local quantum physics” [1] exposes its aims more clearly: incorporation of the locality principles of Faraday-Maxwell-Einstein into quantum physics with the least amount of additional inventions. The global structures, pointing into the direction of global geometry and topology, are to be achieved at the end, and do not belong to the foundations.

At a conference in the early 60’s, Goldberger (a leading expert on the phenomenological use of dispersion relations at that time) once said: “the contribution of axiomatic QFT to physics has been smaller than any preassigned ε .” In retrospect one notes that only those parts of the dispersion theory survived (and are still taught in courses) which were reasonably close to the principles of QFT.

The detrimental and prejudicial effect of that word “axiomatic” is to a large degree responsible for the fact that deep and interesting results of general QFT went unnoticed or got willfully ignored, even if they were very relevant to the main-stream physics (the reader will come across a lot of instances of this kind in the subsequent sections).

But this only explains the situation up to 1980. The lack of interest in pure local quantum physics after 1980 is definitely a result of the exaggerated expec-

tations in the importance of geometric and global topological structures for the formulation of quantum physics. Now, after this development has, to some degree, returned mathematics, algebraic QFT may have the chance to fill the void left behind.

Having indicated in the introduction that I will keep my thoughts free-floating and uncensored, let me add the following. While at the University of Illinois, I once read in the newspaper about the failure of a small tomato-ketchup producer. He had the unpopular (as it later turned out) idea of producing a pure, strong-tasting and natural ketchup from high quality tomatoes, without the standard additives of pineapple juice and chemical ingredients. But most people did not want to miss those ingredients to which they were already addicted.

One of my first contribution in collaboration with Haag [3] was a reexamination of those postulates of QFT which were already studied in the context of local covariant fields by Wightman, but now in the light of a more algebraic setting. We had little problems in convincing ourselves that the observable algebras belonging to compact space-time regions (e.g. double cones) should be indecomposable factors.³ However when it came to question of what von Neumann-type these algebras belong to, we could not find an argument that they are, as in ordinary quantum mechanics of type *I* (although, at least at the beginning we thought that they ought to be). The other types, especially type *III* were already described in the textbooks of those days (we had a German translation of Naimarks book on “normed rings”) but they were somewhat “queer” and very rare indeed. But we were careful enough not to let our prejudices enter our article. Some years later, we learned through Araki’s work [1], that those local algebras are type *III* von Neumann algebras which, in contradistinction to standard Q.M. contain only infinite dimensional projectors. At a summer school in Boulder, Colorado, Irving Segal presented a proof that the local algebras are type *I* factors. Araki, Haag and I were in the audience. We looked at each other with a twinkle in the eyes and I could not suppress a certain feeling of malicious pleasure. If you almost run into a pitfall yourself, you feel less stupid if this happens to somebody else, especially if his reputation is as high as that of Segal. Looking at his calculations more closely, we realized that he did a correct calculation on the wrong algebras (i.e. not the physically relevant causal covariant algebras).

Such an episode on something which, from a physical point of view, appears so esoteric, may hardly seem worthwhile mentioning. Well, it is not really that esoteric. Recently a Phys.Rev.Letter was published [4] on an apparent causality breakdown in relativistic QFT. The main claim was, that Fermi’s calculation on causality and his conclusion that c remains the limiting velocity (even in quantum theory) were wrong. After this article passed the Phys.Rev.Letter referees and the

³ The factorial structure and the closely related duality property were on Haag’s mind already before the start of that collaboration. For this reason, I recently proposed to my colleagues to call it “Haag duality”, a proposal which enjoyed widespread acceptance and also facilitates the distinction to other notions of duality.

issue was taken up by the editor of *Nature*, there was nothing which could stop the making of a new hero who allegedly had shown that there is no principle which prevents “quantum” time machines. For the world press, this was a welcome opportunity to fill their summer lull by something different from “Nessy” of Loch Ness. For physics it was a bit of a scandal of the dimension of “cold fusion” or the alleged outwitting of causality by quantum mechanical tunneling (a closer examination may actually reveal that the recent hoax and the tunnelling stuff can be reduced to the same conceptual flaw).

The mistake was not computational, it was purely conceptual. The author started from the wrong (disproved in the mentioned work of Araki) assumption that the local algebras of relativistic quantum field theory have minimal projectors, (i.e. that local unrefinable observation are possible) like those Heisenberg-Weyl algebras of ordinary Q.M..

This event, more than anything else, casts a strong light on the deep crisis of contemporary particle physics and quantum field theory (with increasing emphasis on the entertainment value of physics ⁴). Perhaps one should consider it more as a social crisis caused by the present generation of theoretical physicists whose mathematical sophistication is either limited to Q.M. or developed too much into the direction of differential geometry and algebraic topology to such a degree, that important conceptual gains in local quantum physics got lost or were not even noticed.

To me it is somewhat sad that notions of causality and localization of states which are at the core of QFT (and condensed matter physics) are so little known or appreciated by the majority of mathematical physicists. After all, the chronology of their evolution is the most fascinating part of nonperturbative QFT. Wigner took his findings on the impossibility of relativistic localization within a one particle space (the Newton Wigner localization [1]) so serious, that he used it as a criticism against the QFT of the 50^s.

Especially those concepts related to localization of states on causal nets really took some time to be conquered. In my view, the most poignant formulation is a fairly recent one by Fredenhagen: a state ω is localized in a space time region O , if outside (i.e. on local subalgebras in the causal complement O') it is dominated by the vacuum state ω_0 . This localization concept, which already appears in a previous work of Buchholz, is not only stable under composition of states (a property shared by the closely related DHR localization), but it maintains this stability even under purification.

It should be added that the causality affair was closed (hopefully!) by a beautiful remark of Buchholz and Yngvason [5] (also published as a *Phys.Rev.Letter*). This paper, although being crystal-clear in its content, is unfortunately very laconic on the history and the background of that issue, and therefore its main

⁴A good illustration is the abstract of the recent paper by R.G.M $\cup \phi$ “The fractional quantum Hall effect Chern-Simons Theory, and integral lattices”. ETH Zürich preprint 94/18

content may get somewhat lost for nonexperts.

While still at Hamburg University, I got to know (mainly through their lectures) Profs. Pascual Jordan and Wilhelm Lenz. Most students know one of Prof. Lenz's contributions via the Lenz-Runge vector in the treatment of the integrable Kepler-2 body problem. The number of people who actually know that he invented (and published in a one page note) the "Ising model" in 1912 (before the Bohr atomic model) and that his student Ising solved the one-dimensional version in his thesis (although from his findings he drew the wrong conclusion that the lattice version of the Boltzmann-Gibbs statistical mechanics seems to be incompatible with a phase transition) is quite small.

When one enters a field as theoretical physics as a student with idealistic principles and learns that the name of the model has little to do with its original inventor, one is a bit shocked. Later one notices that baptizing things by names of people is quite often unrelated to the true history of evolving ideas but rather reflects fashions and sociology and not always the actual scientific content.

Recently I came across a fascinating interview [Atiyah, collected works] which Atiyah gave (a long time ago) to a science journalist. He mentions that the physics Nobel prize has a bad effect on physics. I completely agree with that, since already since the time of Einstein, the Nobel prize has an effect of "vaticanization" (immortality, infallibility, power) in science. The Fields medal however was thought to be different in this respect. After such a long time a physicist would be interested to know whether mathematics was able to protect itself against this effect of vaticanization.

I found Wilhelm Lenz also impressive as a person. My esteem even increased when I later got to know that he was strongly anti-Nazi. He helped and protected Touscheck and continued to lecture on relativity through the second world war. For a short time during the 30's, Pauli was his assistant (at the time of discovery of the Pauli exclusion principle). Apparently Pauli had a totally different personality from Lenz.

I decided to work with Harry Lehmann who, with the strong support of Pauli was made the youngest physics professor at Hamburg in succession to Lenz. After two years, the attempts to learn field theory were bearing some small fruits. In my diploma thesis I was able to characterize free fields solely by two point functions. The proof of this theorem was then put into a more elegant form by Jost (Lehmann told him about my result) and extended by Pohlmeyer [2]. Four years later, when for a short time I shared an office at the University of Illinois with Marc Grisaru, he asked me whether I already know the characterization of electromagnetic free fields via a vanishing current-two-point-function which he proved in collaboration with Federbush and Johnson (I think it was important for Johnson in his short distance studies of renormalized QED). Well, of course I did know about such a theorem.

I was very much attracted by the idea of Borchers that the S-matrix does not distinguish a particular local interpolating field, but rather is an object belonging

to a whole class of such fields [1],[2]. It was of course easy to guess that Borchers' equivalence class of all fields which are relatively local with respect to the free field in Fock-space was just given by all local Wick polynomials including arbitrary space-time derivatives. Although this class belongs to the trivial S-matrix $S = 1$, it was not so easy to prove this guess. The statement (but not the proof)⁵ with references later entered the book of Streater and Wightman, one of the early accounts of general QFT [2].

The insensitivity of the S-matrix against local changes of interpolating fields was understood in a very natural way within the Haag-Kastler net-framework. It served as the first strong hint that the new philosophy underlying the nets of observable von Neumann algebras with their relations and inclusions (i.e. the adaption of Leibniz' ideas about monades and the reality created by their relations rather than the Newtonian viewpoint of a space-time manifold and its material content) was on the right track.

Related to this, one interesting episode comes to my mind. When Haag and I mentioned these flexible properties of interpolating fields (in connection with the axial current and its use as a pion-field) in a private discussion with Gell-Mann, he at first seemed to be very surprised. On the next day he already fully accepted that one does not need a Lagrangian π -field in order to describe pions.

Much later I was fascinated by Gell-Mann's idea that currents alone may already determine the fields. I interpreted this naively (i.e. not in the sense of the DHR representation theory of local observables) as meaning that it should be possible to reconstruct bilocals from composite locals. The idea worked, if one uses light-like limiting procedures [6]. Recently Rehren [7] found an analogous problem in chiral conformal QFT, but in that case he had to invent a different method.

I came to Champaign-Urbana (University of Illinois) in 1960, after having graduated from the University of Hamburg, in order to join Rudolf Haag (who became full professor of physics at the University of Illinois some month before I arrived). I only had a German diploma in theoretical physics and my credentials nowadays would have been considered as completely insufficient for a research associateship. It was certainly not any intellectual brilliance on my side, nor was it solely the high scientific reputation which Haag among colleagues of his own generation was enjoying. The generous funding which physics profited from in the US during the 60's was a result of the cold war arms race in general and the sputnik shock in particular.

Even in this "golden age" of theoretical physics it was not so easy to make ones carrier in such an esoteric (as such it appeared to the majority of theoretical physicists) area as "axiomatic" field theory (a name which I found completely misleading) or in algebraic QFT, where you needed a very good command of von Neumann and C^* algebras (at that time a rather dry looking mathematical

⁵ The proof I share with H. Epstein.

area with little physical intuitive appeal, this was long before Connes, Haagrup, Jones, and others revolutionized that area). Nevertheless the contributions of Araki [1] in these early days were quite impressive, albeit out of reach for me.

At that time (after the aborted S-matrix bootstrap approach to elementary particle physics) there was a strong revival of Lagrangian field theory and renormalized perturbation theory on a more sophisticated level than that of the early 50^s. At one of the meetings on general QFT, I remember Res Jost saying: “Auf Wiedersehen in der Herberge zur Lagrangeschen Feldtheorie”. Many years before, he had said “In the thirties, under the demoralizing influence of quantum-theoretic perturbation theory, the mathematics required of a theoretical physicist was reduced to a rudimentary knowledge of the Latin and Greek alphabets.” I like somewhat provocative statements, especially if they are able to condense the change of Zeitgeist in a perfect way.

Somewhere between Lagrangian and algebraic QFT there was so called “constructive QFT”, [8] which at times tended to be also somewhat destructive. Constructive QFT was too much limited by the idea that one only needed a mathematical control of already more or less existing structures. In other words, one only had to grab into that “unclean” (from the point of view of quantum fields and their short-distance properties) Lagrangian box and mathematically polish some representative models.

It often failed to see new structure (solitons, integrability etc.), but in many cases it succeeded to incorporate those new things.

And, last but not least, there was the newly developing area of exactly solvable two-dimensional relativistic QFT’s of which the first one was the massless Thirring model (and the closely related Luttinger model in condensed matter physics). I moved freely between these areas with a slight preference for two-dimensional models and Lagrangian renormalization theory.

The two-dimensional theories were called either “trivial” or “pathological” by the majority of self-styled “real” physicists. But in order to make a certain specific, conceptually interesting point (capable of a generalization to mathematically less controllable situations like the infrared problem in QED), I found even some of the “trivial” ones useful. In this way I came across the interesting looking concept of “infraparticles” [9] which suggested a new scattering theory outside the LSZ S-matrix framework.

I never admitted the existence of “pathological” two-dimensional QFT (at least if they, unlike the so called generalized free fields, admitted all the known local field theoretic structures with the possible exception of bosonic or fermionic space-like commutation relations). It is now clear to everybody, that what some people called “pathological” at that time was nothing else but : “as free as possible under the constraint of possessing “exotic” space-like commutation relations”. In modern parlance they were “anyons” i.e. fields with abelian braid-group commutation relations and relative bosonic relations with respect to the observable subalgebra generated by them.

The first paper in which an additional parameter in the massless Thirring model was discovered which turned out to be directly relatable to exotic statistics, was that of Klaiber [10]. He showed also that the anyonic statistic parameter determines the behaviour under Lorentz-transformations,⁶ i.e. a new kind of spin-statistics connection valid for that extended Thirring model. In addition one finds half of the “bosonization” in terms of line integrals over chiral currents. Much later this was independently discovered in a more geometrical gauge theoretical setting by Mandelstam, with a significant extension by Coleman. This interesting development had its counterpart for the one-dimensional electron gas in condensed matter physics (the Luttinger model). The hamiltonian bosonization (anticipating the Sugawara form of the energy-momentum tensor for conformal current algebras) was found by Mattis and Lieb before Klaiber’s paper and the bosonic representation of fermions is due to Luther and Peschel. Only after 1964 the two communities took notice of each other.

Lehmann [11] believed that Coleman’s somewhat formal arguments (concerning the infrared aspects of perturbations on massless situations) are only valid in the regime of small Sine-Gordon couplings, and that the bosonization formulas change significantly in the larger coupling regimes. He, together with Stehr, exemplified this suspicion by directly bosonizing the free massive Dirac theory without the use of Coleman’s arguments. In collaboration with Truong [12], I succeeded to understand the mechanism behind this modification of the Klaiber-Mandelstam-Coleman bosonization as a result of the appearance of nonleading short-distance singularities in the larger coupling regimes: “cumulative mass effects”. Later, after the renaissance of 2-d conformal field theory and Zamolodchikov’s successful idea to construct integrable representatives (within the family of field theories obtained by perturbing conformal field theories with relevant operators), it became clear to me that those observations on cumulative effects really mean that the underlying field algebras change dramatically under such perturbations: a (composite) primary conformal field is accompanied by a whole family of “shadow operators” [13] (not conformal secondaries!), which have no place and no name (and do not occur) in the conformal theory. So the naive picture, that perturbation just means some sort of “dressing” via states only (and that it leaves the pure algebraic structures unchanged) does not hold.

Jumping back to the late 60’s, it is worthwhile to recall, that at that time there were many purely theoretically motivated ideas around. There was also a potential richness of mathematical methods since the formalism of QFT was not yet narrowed down to functional integrals (as it has been nowadays by looking at the majority of QFT textbooks).

One problem which attracted my interest was that posed by Wightman [14]: to get a good understanding of stationary and time dependent external potential

⁶A very good account about “generalized statistics” up to 1980 may be found in Swieca’s 1980 XVII Karpacz winter school lectures.

problems for free fields, including higher spin field equations. The algebraic part of the problem was reducible to classical retarded (or advanced) - propagators and if the external potential was leading to a modification of the highest derivatives, then there was a violation of Minkowski-space causality. As anticipated earlier by Pauli and Fierz, later exemplified by Velo and Zwanziger and slightly generalized by us [15], this causality “pathology” is generic for higher spins $s \geq 3/2$.

Many years later, Wess told me that he expects that the restriction to supersymmetric external field problems may yield a higher spin situation which is consistent with causality as well as probability conservation (unitarity). I never tried to verify whether such an exception to our generic findings really was possible. But it seems to me that the failure of supergravity to lead to full renormalizability (of all orders, not just a high-energy improvement in lowest order) would cast doubt on such a conjecture.

As the important part of our external field work we considered the understanding of sufficient conditions (sometimes also necessary and sufficient) for the existence of unitary time development operators and the S-matrix by explicit formulas. Although in our formulation the infinite Feynman phase (the one coming from the “vacuum bubble diagram”, say in a fermion theory) did not appear, it made its reappearance as a finite phase after concatenation of two time dependent processes. These cocycle phases reappear in the later work of G. Segal who uses a geometrically much more appealing mathematical framework (infinite Grassmannians, a more geometric understanding of “filling the Dirac-sea”).

But in physics one is forced by the quantum principles to at least start with C^* -algebra concepts and states on those algebras. From a mathematical physics point of view this problem was laid to rest by Ruijsenaars [17] and Fredenhagen [18]. It would be a grave omission not to quote the profound earlier work by Araki on the representation theory of CAR algebras. In a very recent paper of Böckenhauer [18], this formalism was successfully used in order to give explicit constructions of the endomorphisms (local and global) of the conformal chiral Ising field theory (and to show various equivalences between these endomorphisms).

The extension of the Wigner theory to interacting problems with either external or quantized fields had to be handled with care [15]. In most of the papers which followed Wightman’s external field program this care was observed, but there are also older papers where the central issue was the covariance of Feynman rules for the S-matrix and problems of causality and stability were not yet considered [15].

There was another useful nonperturbative mathematical technique: that of spectral representations (Källen-Lehmann, Jost-Lehmann-Dyson). Swieca used it in a fascinating way, obtaining a non-Lagrangian proof [19] of the Goldstone-Theorem (far away from quasiclassical ways of thinking). Much later he used such spectral techniques in order to prove his “free charge versus screening” theorem [20], a structural theorem on abelian gauge theories. This extremely seminal

paper gave rise to sophisticated studies within algebraic QFT of the relation between mass gaps and the best possible localization of states, and in this way it led to what is now called the Buchholz-Fredenhagen theory [1] (it generalizes the localization aspects of the DHR theory). For a nonexpert it is not misleading to think of charge-carrying objects with the BF-localization as some semi-infinite Mandelstam-string of local gauge theory. However, all attempts failed to obtain an intrinsic i.e. structural insight into what may be behind nonabelian Lagrangian gauge theories.

One should add that techniques of spectral representations were widely used in the 70^s in problems of current algebras, in particular the quantum Noether relations between current densities and symmetry generators. They of course also became standard in condensed matter physics.

The most impressive theoretical discovery of those times was that of the relation between real-time Minkowski-space QFT and euclidean QFT [8]. In a more physically restricted sense it is the relation between noncommutative but local QFT and certain classical (commutative) stochastic theories defined by Boltzmann-Gibbs formulas extended to correlation functions. These two theories were based on completely different physical principles, and yet they had this obviously deep relation to each other. Moreover this relation was a precise correspondence between a quantum theory and a classical theory unlike the various quantization recipes. Whereas quantization for certain finite-degree systems allowed a reasonable good mathematical understanding in terms of geometrical and topological concepts (“geometric quantization”), mathematicians did not find a good “drawer” in which they could place this new correspondence. Some formal aspects were already known to Schwinger, before Symanzik, Nelson, Guerra, Osterwalder and Schrader (and some others) made their important contributions.

Schwinger had a very difficult formalism which, in contrast to Feynman’s simple rules was not overtly perturbative. It could for example handle problems like $\mu\bar{\mu}$ pair production in strong fields (of astrophysical relevance) and unlike the aforementioned external field treatment you did not have to break your head about how to formulate adiabatic boundary conditions (switching on and off external fields): they were already built into the formalism. I never completely understood all that magic, but his former students like Kadanoff, Martin, Baym and Summerfield were very good at handling it.

Schwinger was the first physicist who realized that the \mathbf{Z}_2 of fermions (showing up in the transformation properties as well as in the space-like (anti) commutation relations) can be encoded into the position of euclidean operators (i.e. Grassmann algebras instead of analytic monodromies in analytically continued correlation functions).

Much later, Marino and Swieca [21] (in their functional integral treatment of order-disorder variables and generalized statistics) tried to generalize this to the e.g. \mathbf{Z}_3 structure of $s = \frac{1}{3}$ which occurs in 2-d models. In order to encode this into pure algebraic terms, one would have to invent multivalued algebras, and

no standard linear way of writing products (neither Latin nor Hebrew) would allow such an exotic structure (one would have to write into several directions simultaneously).

I was reminded of these interesting discussions I had with Swieca, when I attended a recent seminar talk of R. Kerner with the title “Ternary structures and new models of gauge theories”.

It is hard to imagine that Schwinger, who was a quantum realist, would have been interested in “Berezin integration” or other attempts of geometric quantization, in which one invents a classical reality just for the benefit of being able to say: I quantized something. However I can imagine that the exploration of perturbative ε -neighbourhoods of free bosons, fermions and plektons (explained later) could have attracted his interest, especially if (as it happens in some cases) a global control is possible.

That part, which had to do with the more profound aspects of “euclideanization”, however, became only clear in the work of the previously mentioned authors. Under fortunate circumstances (proximity to a ϕ^4 model theory), the corresponding euclidean theory admitted a Feynman-Kac functional integral representation. The other way around the problem is more difficult. With other words, you could not simply define a noncommutative QFT by such stochastic Feynman-Kac type integrals, without going through a check-list of rather difficult properties. Only after you have done this, you are assured of a quantum theory. The mere existence of the infinite-dimensional stochastic integrals is not enough (and in case of nonexistence, one may still have “structural correctness” of the F.K. representation after renormalization, as it happens for ϕ^4 in $d = 3$ or 4 space-time dimensions). The separation of structurally correct functional integrals into algebras (the “Faraday-Maxwell-Einstein” local part) and states (as a result of coherences and correlations necessarily nonlocal) belongs to one of the most subtle procedures of QFT. We will return to this interesting issue in connection with “topological QFT” via Chern-Simons actions in the next section.

In this context it may be interesting to make the following remarks. In the process of conversion of functional integrals and their geometric structures into quantum physics, the BRST formalism is supposed to play an important role. Whereas this is certainly the case in perturbation theory (this is the origin of this formalism), one can have serious doubts about its widespread non-perturbative geometric use (these doubts I share with Stora, at least when I met him the last time some years ago). “Free” fields (including integrable and conformal fields) do not care much about the Hilbert-space structure,⁷ they are also compatible with indefinite metric. However, it is very improbable that interacting theories with indefinite metric can be controlled by such methods. I do not know any trustworthy mathematical framework which allows us to do that. The use of the Gupta-Bleuler formalism for recovering positivity is perturbative-inductive.

⁷Integrable and conformal models can be mathematically controlled by only using locality.

Having said that I should not hide the fact that I once committed a sin against these principle of never touching gauge dependent objects outside integrable (solvable) models [23]. There was the issue of “screening versus confinement” which was important even in 2-d models [24]. The massless Schwinger model exhibits screening since a gauge invariant version of “quark ” fields existed (a Dirac-spinor with a semi-infinite Mandelstam string), but it was neutral. It was clear that the massive version (which is not solvable) behaves significantly different. The most radical expectation was that such confining quark operators cease to exist as mathematical objects, since there is no physical role for them. We (in collaboration with K. Rothe) succeeded to show that the long distance behaviour of quark fields in the Lorentz gauge is indeed terrible, the correlation functions are non-tempered for long distances. This was done by a tricky use of special nonperturbative methods and, in agreement with the previous remarks, there was no chance to use BRST for this non-integrable model. I still think that there is some intrinsic message behind this, but I have not been able to liberate it from the gauge stuff. As far as “sinning” against the above principle, I found myself in the good company of Strocchi and Wightman and also of Fröhlich.

The real-time-versus-euclidean relation was certainly the cleanest and most useful result coming from constructive QFT. It was primarily a structural insight which did not immediately facilitate an analytic understanding of nonperturbative QFT. However, it gave an excellent framework for the formulation of renormalization group ideas either à la Wilson and Kadanoff or in the more field theoretic spirit of parametric differential equation à la Callan-Symanzik and Gell-Mann-Low. With the help of ordinary renormalized perturbative QFT and a trick of analytic continuation in the space-time dimension (the ε -expansion with $d = 4 - \varepsilon$) one could (even without knowing very much about the physics of critical phenomena) produce some surprisingly good numbers. Even though I contributed one of the first papers [22] after Wilson, I still do not understand why these methods deserved such good numerical results.

Already in the early 70^s there was some hope that if one combines the acquired knowledge about the real-time-versus-euclidean connection together with conformal invariance, one may be able to obtain explicit nonperturbative solutions or at least to find some detailed classification scheme of such theories. These ideas originated from two observations. One observation was that two-dimensional statistical mechanic models (as the Ising model solved by Onsager and Kaufman) at criticality showed not only scale-invariance, but even full Möbius-invariance, at least to the degree to which one could understand such concepts on a lattice. From continuous renormalized perturbation theory one already knew that the additional conservation law which results from being at a Gell-Mann-Low fixed point does not only imply scale invariance but also full conformal invariance. Based on these observations, Migdal and Polyakov suggested a “bootstrap program” for conformally invariant theories. Mack and Symanzik [25] showed that such a program basically amounts to nonlinear Schwinger-Dyson equations

with conformally invariant boundary condition. Mack [26] developed a group theoretical technique of euclidean conformal partial wave decomposition, but the nonlinear “dynamite” remained resistant against an analytic solution for another ten years.

The second observation was that there are nontrivial massless solvable real-time models like the Thirring model. Their observable currents are conformally covariant in the standard sense, but there was an apparent obstruction against global conformal invariance in the charge carrying fields. This obstruction was easily related to a breakdown of Huygens principle, i.e. a kind of time-like “reverberation”, showing up in the fermionic time-like commutators. Since this contradicted our [27] naive expectation about global conformal transformations leading from space-time via light-like infinity to time-like, and since it was not visible in the infinitesimal behaviour, we called it the “Einstein causality paradoxon” of globally conformal invariant QFT’s. We were convinced that this obstruction is worthwhile to resolve, since it was not there in the euclidean approach and since in our view the real-time-euclidean connection was very deep and, as especially the Nelson-Symanzik work showed, not fully described by only thinking about analytic continuation. So we expected to get an additional structural property which remained hidden to the “euclidean eye”.

Indeed, when Swieca and I finally solved this paradoxon two years later [28], we had an extremely rich harvest: a conformal decomposition theory of local fields which, when specialized to two dimensions, is equivalent to the block-decomposition theory of Belavin, Polyakov and Zamolodchikov. The local fields which are irreducible under infinitesimal Möbius-transformations, turn out to be reducible with respect to the center of the covering group for $d = 2$, and the center (as well as the whole group and the theory) factorizes into the two chiral parts. BPZ, who worked in the euclidean framework, had to obtain this additional piece of information (which resulted in our case from the resolution of the paradox) by the explicit use of the representation theory of the Virasoro algebra.

Either way, this was the “magic” by which the “nonlinear dynamite” of Schwinger-Dyson structures became linearized in two-dimensional conformal field theories.

Our findings in 74/75 confronted us with a very perplexing situation. We thought that the Wightman theory was a universal framework of local fields, but these new non-local-looking fields, which came from the decomposition of the local ones with the help of central projectors (between which the local field get “sandwiched”) were not strictly speaking Wightman fields. They violated the Reeh-Schlieder theorem since they had huge null-spaces (i.e. they are only nonvanishing if applied to appropriate superselection-sectors).

As already mentioned, with a rich and partially-known representative theory of the Virasoro algebra at their hand, and with the significant insight of Kadanoff into the structure of critical points by the “Coulomb-representation”, BPZ in 1984 obtained the first really nontrivial (non-abelian fusion laws) family of “minimal”

models. We in 1974, lacking such powerful analytic tools (which allows one to bypass or postpone a lot of conceptual problems), had only the well-known abelian family of exponential bose fields (known from bosonization) at our disposal [28] which are too poor to serve as good illustrations of the richness of our conformal decomposition framework.

In the second half of the 80° when Rehren and I looked back at the old things in order to understand their relation to the BPZ discoveries, we convinced ourselves that by classifying statistics (i.e. R-matrix commutation relations) or fusion laws, and computing from that dimensional trajectories of composite fields, one can obtain a situation with a unique solution of a Riemann-monodromy problem for the 4-point function by not using those BPZ tools but rather standard methods of QFT. This shows that in principle we could have done it in 74, but, “like Gorbachev used to say” – who comes too early (and is stained by “axiomatics”) is punished by life.

Because of these conceptual difficulties posed by those exotic looking fields and as a result of a strong prejudice in favour of euclidean Feynman-Kac representations (I am nowadays free of that) we gave up in 75 and turned to other problems which were more in the vein of the times. But it is worthwhile to add two remarks. In January 1974 at the V Brazilian Symposium on Theoretical Physics, I presented a theorem [29] on the “Lie-field” structure of the chiral conformal energy momentum tensor. I was totally unaware of previous work of Virasoro who found the same algebraic structure by solving a constraint problem which appeared in Veneziano’s dual model. His algebra served as a kind of Gupta-Bleuler condition removing unphysical degrees of freedom. I never used a nonlocal Fourier-decomposition. Its use was more natural from the compact picture description of string theorist, but a bit unnatural from the point of view of critical statistical mechanics or QFT.

My motivation came from Lowenstein’s previous attempts to classify “Lie-fields” by their space-time commutation relations. Lowenstein’s program was probably a precursor of the W-algebra program. In any case, the conformal energy momentum tensor was, if I remember correctly, the first really interesting and explicit example of a Lie-field in the sense of Lowenstein. In the same publication I also studied differential identities coming from short-distance expansions of chiral currents with fields. Such differential identities are special cases of what nowadays is called Knizhnik-Zamolodchikov equation. I used them for the rather modest aim to show that the new solution (at that time) of the “generalized $U(n)$ massless Thirring model” proposed by Dashen and Frishman [30] was not really new, since one could obtain it by interchanging the role of Euler-Lagrange equations (which have a classical limit) with those differential identities (which are of pure quantum origin) but just starting from the old solution.

Those conformal models which are nowadays called WZWN-models in those days were called generalized Thirring models with their affiliated (nonabelian) current algebras.

Whereas I can reconcile myself easily with the terminology in the first mentioned case, because Knishnik and Zamolodchikov significantly enriched the old physical structures and produced new results, I have some problems with the WZWN terminology in physics (but not with the authors!). The reason is that neither is there a close connection between those Lagrangians in lower dimensions which have the same (or in this case a similar) algebraic form as the higher dimensional phenomenologically motivated WZ models (the vast difference between the Schwinger model and 4-d QED may serve as an example), nor can such nice looking geometric Lagrangians through their Feynman-Kac integrals be dealt with in any convincing and efficient way, even if these integrals would exist as mathematical entities. There is no such problem with the old terminology, especially if one treats such generalized Thirring models and their associated current algebras with the help of Knishnik-Zamolodchikov differential identities (the classical Euler-Lagrange equations are not enough for an efficient analytical control). Already in the late 70^s it was clear that the non-abelian Schwinger-determinant (the fermionic determinant in a generic nonabelian gauge field) was an interesting object. We computed it in the Pauli-representation [31]. Using a particular parametrization for the gauge potential (the Polyakov-Wiegmann representation of 2-d gauge configurations in terms of sigma-model variables), the Pauli-parameter is turned into a “third dimension” and thus the Schwinger determinant becomes local [24]. Its logarithm was some years later called the WZWN action. The natural development would have been the use of that determinant for the nonabelian version of Coleman’s bosonization of the U(n) Thirring model (the general Thirring model with arbitrary multicomponent quadrilinear and bilinear terms is one of the richest models for $d = 2$ phase transition with many conformally invariant points in parameter space). After all the indications that this ⁸ may lead to interesting conformally invariant points (or submanifolds in the general Thirring coupling constant space) already existed [30]. But the more quantum physical ideas of the seventies got disconnected from the WZWN geometric quantization approach [32] of the 80^s. Actually Witten connected the model with conformal current algebras, which can be interpreted as being half-way towards a nonabelian Coleman bosonization of the nonabelian Thirring model. A complete treatment may also reveal that anomalies and obstructions are not intrinsic properties, since the generalized Thirring model does not seem to have them.

In conformal QFT the use of the new terminology is more ideological (in the spirit the first footnote) than computational (everybody uses old fashioned current algebras for computations). The terminology sacrificed the relations to the generalized Thirring model, one of the physically most important class of

⁸using e.g. the quadratic completion functional trick to convert the generalized Thirring model into a trilinear gauge interaction, just as one does the functional bosonization of the abelian Thirring model with the help of the usual Schwinger determinant

two-dimensional models, in favour of geometrical prowess. This is not the only time that physical insight was traded for “geometrical beauty”.

Algebraic QFT is one of the few areas where an impressive caution is observed if it comes to terminology. In case of new concepts, the terminology (at least in all cases I am aware of) is extremely clear and physically appropriate. Take for example the notion of “statistical dimension” in the DHR theory. It immediately reveals its meaning as an amplification (or multiplicity)-factor resulting from particle (or field)-statistics. It is precisely in this way that an experimentalizer measures the effects of symmetry (amplification factors in cross-sections) even before a theoretician tells him how to interpret it. Compare it with the word “quantum group” or “quantum dimension”. Here (apart from the misleading word “group”) one does not know whether “quantum” is meant in the sense of Planck and Heisenberg (discreteness) or in the sense of Flato, Lichnerowicz and Sternheimer (deforming Poisson-brackets to brackets of noncommutative operator algebras). The latter meaning is not intrinsic,⁹ one will never see a quantum theory running around with a tag “I come from geometric quantization (or q -deformation)”.

This precision and depth as well as an awareness of history one also finds in pure mathematics when it has not been influenced by fashion from physics. The name “Markov trace” is an excellent illustration. It combines the Markov of this century (the topologist) with his father (the probabilist). Compare this with the above re-naming of the nonabelian Schwinger determinant.

Times of crisis usually reveal themselves (ever since the famous tower of Babel) through a sloppiness in terminology.

Apart from its intermediate role in functional tricks of nonabelian bosonization, QCD_2 is a fascinating quantum physical theoretical laboratory in its own right. It was never solved as its abelian counterpart was, but by using (uncontrollable) approximations it was used for the study of possible confinement-mechanisms. In my view, however, it should serve for the opposite mechanism: the liberation of Coleman half-space kinks. I was unable to understand such a mechanism in QCD_2 , but in the less interesting case of a generalized Schwinger model (still abelian) an argument was found [33].

When Fredenhagen recently elaborated a general algebraic framework for half-space kinks [34], I was reminded of those old attempts. Since the Fredenhagen theory is the one-dimensional version of the Buchholz-Fredenhagen semiinfinite strings (or rather space-like cones), which everybody intuitively interpretes as the quantum version of the Mandelstam strings of semiclassical gauge theory, my conviction that QCD_2 is the theory of liberated Coleman-Fredenhagen half-space kinks was strengthened. With the help of some formal progress coming from conformal QFT, one should be able to understand this problem. The progress

⁹If we got used to such weird terminology as “an applied mathematician” (or physicist!), we might as well accept the above terminological ambiguities.

could result from using (in addition to the Euler-Lagrange equation) those previously mentioned differential identities but now in their gauge covariant version, which leads to a much richer algebra. I think that if somebody revisits those old problems with the hindsight of these new ideas, he may be rewarded by a rich physical harvest.

It may be interesting to add the following remarks. The properties of Coleman-Fredenhagen half-line kinks are closely related to particles which are statistical “schizons” [35]. The simplest example of this phenomenon is provided by the two-dimensional massive free Dirac theory. In its Hilbert space one finds a bosonic field which, although being very nonlocal with respect to the Dirac field (viz. the kink-like commutation relations) still is local relative to the observables generated by the current operator. This field interpolates the same Wigner-particle as the Dirac spinor, i.e. the particle is a statistical schizon. This phenomenon is related to the fact that the algebra generated by the current does not fulfill Haag duality ([1], III 4.2, where the free Dirac current is discussed). The various possibilities for Haag-dual extensions of this algebra give a powerful tool for a future more profound understanding of this “schizon” phenomenon. Note that it is not to be confused with the bosonization-fermionization formalism in massless theories. In the latter case the statistics of fields is fixed by the superselection structure and one only changes the description and not the content.

In 1976 I spent a short time in Hamburg. Lüscher and Mack, knowing my work and my interest in “Lie fields” and the conformal energy-momentum tensor, showed me some incomplete results on the representation theory of the algebra generated by the energy momentum tensor. They had the beginnings of the c -quantization for $c < 1$ in their hands. I do not remember how far they went beyond the Ising value toward $c = 1$. Their paper, which went unpublished is sometimes quoted for contributing the structure theorem of the energy-momentum tensor. But (to the degree that the quotation of not generally accessible unpublished work is helpful at all), it should be quoted as a precursor of the Friedan-Qiu-Shenker-work of 1985 because that was precisely what their new contribution consisted in. Only much later, after I read the FQS paper, I could fully appreciate what Lüscher and Mack in 1976 had in their hands. The FQS paper is the perfect example of deep analytic work (using the mathematics of V. Kac) and superb knowledge of the physics of critical 2-d models blended with a very good use of computers. However, knowing the end of the story, one is also able to admire its “dawn” in the unfortunately unpublished LM work.

Apart from these very special (and perhaps even somewhat premature) algebraic contributions, the decade of the 70^s, as far as mathematical physics was concerned, was definitely that of the functional integral and the geometric and topological structures which it suggests. I fully understood the enthusiasm about those new things, since I shared it. My reasonably good knowledge of two-dimensional models, especially about the Schwinger-model through the 1971 paper of Lowenstein and Swieca [36] (who used operator-algebra methods) gave

me an easy start. I was obsessed by the idea to redo everything within the new setting of euclidean functional integrals, paying utmost attention to winding numbers etc. In collaboration with N.K. Nielsen [37] and with some suggestions from Swieca, everything worked perfectly (a little bit too perfect!) : one could explicitly solve the zero mode euclidean Dirac equation in a generic abelian gauge configuration, see the relation between euclidean spinorial zero modes and winding numbers explicitly in front of ones eye, derive the effective action as a conspiracy between these zero modes and corresponding expressions in the modified fermionic determinants, and in this way cast a new (more geometric) light on the origin of the Lowenstein-Swieca θ angle and their description of the spontaneous (in the sense of Higgs-Schwinger) massive breaking of chiral symmetry and the connection with cluster properties (irreducibility of the θ -vacuum). The Schwinger model was (apart from some slight generalization) the only model for which you could do such things (as solving Dirac equations in generic gauge field configurations). Since the methods of constructive field theory for calculating fermion determinates are incompatible with the presence of winding numbers, we used of course the ξ -function method. We were attacked by Patrasciou and Seiler. In spite of the fact that in those models everything was explicit and therefore you could see in front of your eye that no principle was being violated, we painfully wrote up our arguments in form of a short publication and we thought that a title like [38] “Still more about...” would help to close the issue (it did not).

At that time (75/76) I was totally ignorant about the Atiyah-Singer index theorem, although I had the conviction that there was something general behind, maybe even already known to mathematicians. The CERN library was not good for mathematics, so after some time I went to the mathematics library at Geneva University where I eventually found the articles by Atiyah and Singer and I copied some. It took me some more month in order to obtain a first incomplete understanding. The help of Römer [39] (with whom I also discussed some applications to gravity) was essential in order to convert this into a working knowledge. Before I knew all these things and with only my (and Nielsen’s) model calculation at hand, I gave a talk at a small meeting in Copenhagen with some speculative remarks at the end for which I did not have a proof, (not even good arguments) only to be reprimanded by Jackiw.

Probably other physicists had similar ideas, but in those early days nobody had a overall view of what was going on.

As far as model applications of functional integrals and zero modes are concerned, the book by the Abdallas and Rothe [24] may be a good source for information. This does of course not cover the later more mathematically motivated papers of Witten, A. Schwartz, Zumino, Stora, Alvarez-Gaumé and many others, in which a profound topological understanding of anomalies was the central issue.

Although I still followed these (especially geometrically) beautiful developments, I did not actively participate and this was not only because some of these things were beyond my intellectual horizon. Rather I felt that there was an in-

creasing gap between geometry and real quantum physics. I remember talks by Hirzebruch in which he explained the relations between mathematics and “what physicist do” (he did not say “physics”), but I am not sure whether he was just referring to the lack of mathematical precision at that time (which is of course a less important issue in theoretical physics where conceptual clarity is the important goal) or if he really knew that physics is not always identical with what physicists are doing. The relevant question would have been of course: how come that such a deep mathematical theory as the Atiyah-Singer index theory has such an indirect relation to physical observables?

The index theory was applicable to euclidean functional integrals, but e.g. the relation of zero modes to the physical issue of spontaneous symmetry breaking, the Higgs-Schwinger mechanism, the condensation of chirality and disorder fields as well as the restauration of cluster properties etc. was too indirect. In addition the quantization of the classical (fibre bundles etc.) non-abelian gauge theory was seriously impeded structurally by (even forgetting problems of existence etc.) the Gribov obstruction.

Now, with the hindsight of more recent developments, I think I have a partial answer to this question of why the relation of quantum physics to geometry is so indirect. The reason is that most of our quantum intuition comes from theories which allow a “first quantization” with all its classical and geometric aspects. The Heisenberg-Weyl algebra (Bosons) and even the CAR-algebra (Fermions), especially with its “reading back” into classical physics via Grassmann variables, certainly allow for a “geometrization”. But the direct application of geometrical ideas may be limited to this class of theories (this limitation seems to be shared by functional integrals i.e. the question of Feynman-Kac representability). This is at least the impression which one gets from algebraic QFT if one combines the ideas of Haag, Kastler and Borchers and the DHR work with the more recent Jones index theory. Physically such issues as states and their decomposition theory, charges and their fusion and decompositions and statistics of particles (in condensed matter physics also: universality classes of quasiparticles) are more important than geometric or topologic assumptions. This is not to say that useful geometric structures may not result from quantum physical principles. But physical principles always have priority [40]. I will return to these questions in the next section.

One important development, closely related to the remarks before, ought to be mentioned here. Already before the time when winding numbers and A-S zero modes entered physics, it was clear that in case of breakdown of the conservation of the observable chiral current via a Higgs-Schwinger mechanism, the *raison d’être* for the appearance of Nambu-Goldstone boson was lost and the natural generic situation was to find a massive meson (carrying similar quantum numbers) instead. In fact the perturbative Higgs description allowed to call this a “fattened Nambu-Goldstone boson” (with Schwinger’s composite mechanism this terminology is less appropriate). Crewther [41] tried to use the euclidean

zero modes in order to make these arguments in favour of an η -meson more quantitative. He could not quite confirm the expectation and he built a kind of counter-philosophy based on his calculations, only to get into a controversy with Coleman and t'Hooft. The structural results of real time QFT like the H-S mechanism were simply more trustworthy than results obtained by euclidean functional integrals with the help of the A-S index theory. I found this a very interesting and profitable controversy.

Since string theory is conspicuously absent in these notes, I owe an explanation to the reader why I consider it as part of a mathematical rather than a physical-conceptual enrichment. In fact the dual model and the old string theory had strong physical roots: they constitute the first proposal as to what a non-perturbative strong-interaction S-matrix could look like: there are infinite particle-towers which in order $g \geq 2$ are turned into an infinite tower of resonances (poles in the 2^{nd} Riemann sheet). According to my best knowledge, there is no principle in local QFT which could forbid such a situation (i.e. there seem to be no intrinsic “stringiness” of such a situation). But then came the violent end: the Bartholomew-night-massacre of the old string theory by a semantic trick. The new string theory, after many years, got a nice geometric wrapping, but to me it lost its physical credibility. Also I was disappointed when the attempts about string field theory were never directed towards the important problem of what really happens with Einstein causality, which is the soul of standard relativistic local quantum physics.

The pitfalls I ran into with supersymmetry were of an entirely different kind. The observations which Witten made on supersymmetrically degenerate vacua and Nicolai’s functional map (whose degree was equal to the Witten index or the vacuum degeneracy degree) were very interesting indeed. The map was the most unconventional way to obtain free fields which I have ever seen. The relation of that free field to the Heisenberg fields was extremely nonlocal. Scattering theory provides one with interpretable free fields, which are also extremely nonlocal with respect to the interacting fields. But for the Nicolai free fields I could not find a physical interpretation.

I also had some difficulties with the vacuum degeneracy picture. In general QFT there is a theorem which allows to decompose such a degenerate situation into a description with just one vacuum for each component. One then expects an intrinsic property of each component which reveals that it is coming from such a construction. I could not find such a property, and I think there is none. Therefore supersymmetry appeared to me as a mathematical marriage of fermions and bosons without a visible physical match-maker. It is possible that I have overlooked something here.

3 Recent Developments in Algebraic QFT

The bulk of the theoretical development I am getting to now appeared through the 80^s and 90^s. But important ideas in most cases already existed in the 70^s.

The most remarkable one is that of integrable field theories and their S-matrix bootstrap construction. Already at the beginning of the 70^s certain nonrelativistic classical field theories were shown to be integrable, both in the sense of having infinitely many conservation laws, as well as that their scattering theory was explicitly computable (i.e. integrable) by some nonlinear generalization of Fourier-transformation (inverse scattering method). The first indications that certain relativistic theories have a particle spectrum which (similar to the hydrogen atom) seemed to be susceptible to “exact” quasiclassical considerations came from the work of Dashen, Hasslacher, Neveu, Jackiw, Faddeev and many others. If this was interpretable as integrability, there should be conservation laws preventing the creation of particles e.g. in the massive Thirring model. So this should show up as a structural property in every order to on-shell perturbation. Berg, to whom I was an adviser, showed the numerical absence of scattering in $2 \rightarrow 3$ at a certain value of rapidity of the Dirac particle in that model (he used a pocket computer). The corresponding analytic calculations were only done half a year later, the combinatorics (even in lowest nontrivial order) which led to this on-shell “conspiracy” was horrendous and one first had to find clever tricks.

Faddeev with coworkers, [42] with a lot of hindsight and previous work, found a closed exact formula for the massive Thirring-model S-matrix at those values of the coupling constant where backscattering was absent.

In collaboration with Truong and Weisz, [43] I was able to show that in case of the Sine-Gordon equation the trigonometric DHN quasiclassical mass formula for the breathers is a result of the physical principles of unitarity, crossing symmetry together with the assumed absence of pair creation. This was done without using the Yang-Baxter equations which we did not know yet (for that simple scalar scattering states one can write down the factorization into 2-particle elastic scattering without that knowledge).

Then Zamolodchikov [44] found a clever trick to extrapolate the above mentioned S-matrix to the generic case with backward scattering. By a very nontrivial combination of Zamolodchikov’s result (as a check) with a generalisation of our rather simple use of the bootstrap idea in that mentioned scalar case, Karowski et al. [45] liberated the factorizable bootstrap program in complete generality. By that time Faddeev obtained the same structure (which he called “Yang-Baxter”) coming more from solvable models à la Baxter in statistical mechanics.

After that, things really got boiling. The Moscow group, notably the Zamolodchikov’s used that program and found a tremendous wealth of integrable relativistic models, and there were innumerable fascinating discoveries made by Faddeev and his school. Witten and Shankar showed how one can completely solve models (some of them supersymmetric) by calculating the full S-matrix including that of

charge-carrying solitonic states. Swieca sent his student Kurak to Berlin in order to learn the “bootstrap” trade (which he did in a short time, he together with the people at Berlin made up tables where factorizable S-matrix were classified according to symmetry properties [46]). Together with Swieca [47] he later wrote a nice little paper showing that the antiparticles in the Z_N and the chiral $SU(N)$ Gross-Neveu model are really bound states of $N-1$ particles. This in turn fitted neatly with Witten’s idea how via infrared clouds ¹⁰ the continuous symmetry breaking (showing up in the quasiclassical treatment of these models) is evaded in spite of the presence of a mass gap in the S-matrix (the Kosterlitz-Thouless mechanism), one of the few instances of theoretical physics at its best.

As a result of my collaboration with Swieca, I was very much used to see solitons (in the sense of new physical charge-carriers in a theory which did not possess them from the beginning) and confinement (in the sense that a Lagrangian discription may show *more* formal charges than the physical Hilbert space has sectors, as in nonabelian gauge theories) as two opposite sides of the same coin. So if, within the bootstrap framework of factorizable models, you got very good examples for solitons, then perhaps there are also nice illustrations of confinement. From Wigner’s work on symmetries in quantum theory one knows that the important difference to classical symmetries is that quantum theory (as a result of its projective nature) always asks for the covering group \tilde{G} of a symmetry group G of automorphisms of observables. So the natural state of affairs in QFT would be that particles (at least the fundamental ones) also carry the central charges of \tilde{G} . One only had to look for integrable models like the $O(3)$ model where this did not happen and understand this “exceptional” behaviour in terms of confinement. When I discussed this idea with Kurak, he told me that Karowski had already made the formal observation that the $O(3)$ S-matrix was related in a very simple way with the $N=2$ S-matrix of their $SU(N)$ table. When Karowski returned to Berlin from a trip, the paper [49] was already almost finished, he only had to correct some sign related to the parity of bound states. It is well-known that this model also admits a gauge-theoretic Lagrangian description, which is useful if one wants to study its quasiclassical aspects.

A short time later, Karowski [50] published a very seminal paper with a systematic study of bound states and fusion within the framework of factorizable QFT, paying special attention to quantum probability conservation (positivity of residua of boundstate poles). This paper explained, on a much more fundamental level than before, the origin of those trigonometric mass-trajectory formulas (known since the DHN quasiclassical work).

In fact, with hindsight of more recent developments, their origin is formally the same as that for the statistical dimensions in the DHR treatment of low-dimensional QFT or that of the size of subfactors with Jones indices smaller

¹⁰By an additional Schwinger-model coupling, these clouds are “eaten up” by the Higgs-Schwinger mechanism [48].

than 4: the fusion laws give rise to the Perron-Frobenius problem. This paper, via its later extension by Kulish, Reshetikhin and Sklyanin [51], led directly into quantum groups.

Drinfeld discovered (in a mathematical sense) that the original Yang-Baxter equations, extended to a system of Yang-Baxter equations for all particle states (i.e. including the fused or “cabled” bound states), lends itself to be interpreted as the representation theory of a kind of q -deformed group. This was done in a general algebraic setting without paying attention to the probability conservation of quantum theory.

I am not quite sure, whether this development was altogether healthy for theoretical physics. For a theoretical physicist, the ability to manipulate quantum groups and q -deformations (meanwhile of “everything”) without having a profound knowledge of QFT, is a dangerous enterprise, reminiscent of that previous quotation of Jost. Positivity is not such an important issue in statistical mechanics. In the formulation of Jimbo, its Yang-Baxter and fusion roots in the spirit of Karowski and Kulish, Reshetikhin and Sklyanin remained more visible. Woronowicz got his “pseudogroups” within a C^* algebra setting in a completely intrinsic and independent mathematical way.

According to my best present knowledge, the q -deformed group at generic q -values was never incorporated into the quantum physics (Hilbert space, operators) of the Sine-Gordon equation. Rather (similar to algebraic QFT) it was observed, that certain objects (Markov-traces and their ensuing R-matrices), which one can obtain by reconstruction from quantum groups at roots of unity (and which do not show the pathologies of quantum group at those exceptional values), are identical to structures which one attributes to the so-called “reduced” Sine-Gordon model (at quantized values of the Sine-Gordon coupling constant). The present widespread use of q -deformations in “physics” is not covered or supported by such observations.

In the study of superselection rules of low-dimensional QFT’s, quantum groups are only useful at roots of unity, when they give rise to Markov traces on the ribbon braid group algebra CRB_∞ . But the latter can also be computed without quantum groups, using only physical concepts. The Markov traces lead to unitary representations of CRB_∞ , but unfortunately (at roots of unity) in the reconstructed version they “lose their memory” about to which dual quantum symmetry they want to belong to (at least for the time-being). The way back from Drinfeld via Kulish et al. to Karowski’s particle fusion is analogous to the use of q -deformed groups in order to obtain Markov traces. I suggest to the reader that he permits himself this journey into the past because it is physically very profitable.

The name “Yang Baxter” chosen by Faddeev, is of course historically completely correct. But Yang’s work on δ -potential scattering theory (one should not completely forget Mc Guire and Brezin’s et al. contributions at the same time, but Yang’s seem to be the clearest) was probably not known by Faddeev at the

time of the bootstrap discovery (he certainly knew Baxter's work). Karowski et al., on the other hand refer to that Yang's work in their very first paper, but they did not know Baxter's work at that time. There is however an essential amount of QFT in that game, and a young newcomer should be aware that Yang's work would have remained an isolated piece of low-dimensional potential scattering theory, and Baxter's work would not have found the proper central position in quantum physics without these fresh quantum field theoretical developments.

I use the name Yang-Baxter in connection with θ -rapidity dependent S-matrices, but if it comes to R-matrices describing braid group statistics, I prefer the name Artin. It is true that formally the latter one is obtained from the other by $\theta \rightarrow \pm\infty$ (after rescaling), but from a physical conceptual point of view there is an enigmatic distinction. Statistics of particles is a long range phenomenon (in principle one can study it on a lattice). Even though in 2-d physics (in contradistinction to 3-d) particles cannot generically be described in terms of statistics (i.e. statistics is not a generic attribute of composition of 2-d charges) it is reasonable to follow Swieca [47] and distinguish between an analytic S-matrix fulfilling a Yang-Baxter equation and a "physical" S-matrix where the discontinuity in the θ variable at $\pm\infty$ is placed at $\theta = 0$, in order to comply with the LSZ properties and the long-range nature of statistics.

Jones is one of the few mathematicians who makes a similar distinction; in his terminology there are the R-matrices which appear naturally for large families of subfactors, and there is the program of "Yang-Baxterization" which, for the time-being, has not such a clear conceptual placement in the Jones subfactor theory.

At the time when Karowski and Thun [52] investigated the fusion-, spin- and statistics structure of Gross-Neveu $O(N)$ -model solitons, the University positions of Weisz and later also Thun (Berg left immediatly after his PhD) expired and the resulting existential problems led to a decay of the whole group. Research on 2-d field theory at that time in Germany was not exactly a carrier-building activity. A whole group of very bright physicists never got a professorship in Germany.

I remember one, at that time very hard-looking problem which Weisz raised in connection with their work on formfactors. He realized that some of the formulas which appeared in this work were identical to formulaes in some statistical mechanic work of Lieb. Lieb appeared in Berlin quite frequently and so at the next visit Weisz asked that question. Lieb also did not have an explanation. This problem was finally solved by Zamolodchikov, and its solution brought QFT and statistical mechanics still a little bit closer.

One of the most seminal papers on two dimensional QFT's of the 80's is the already mentioned BPZ work on conformal QFT. With the powerful tool of representation theory of the Virasoro algebra, this time interpreted as the Fourier-components of the energy momentum tensor (the original interpretation in terms of a constrain algebra was maintained only in string theory), they calculated a

fascinating new family: the minimal models. This work was significantly extended by Capelli, Itzykson and Zuber, and further developed and explored for the benefit of critical phenomena by Cardy. In this post-BPZ work, the concept of modular invariance by Witten and Gepner, which came from string theory, played an important role.

Around 1986, I thought that it would be worthwhile to develop this (in the spirit of the 74/75 work of Swieca and myself) more into an algebraic direction, with the hope to “liberate” some of the ideas from their narrow conformal compound. In our old work, we did not make the relation to the DHR theory of superselection sectors although (we were both closely linked to Haag at one time or another) we knew its main physical motivation and certain technical aspects. The only physicist, who had the courage to use this in spite of the low reputation (“pathological”) which low-dimension QFT’s were enjoying at that time, was Fröhlich [53]. It is somewhat ironical that we did very similar things (unfortunately sometimes ignoring each other) but at different times. When he worked on algebraic aspects, we (following the order-disorder duality ideas of Kadanoff) looked at euclidean functional representations for sector-creating relativistic soliton correlation functions (we found them in the form of generalized euclidean Aharonov-Bohm representations [54]) and when he [55] studied the euclidean approach to solitons, I was at algebras. But on the issue of braid group commutation relations we both met, he with the personal knowledge of Jones work and the contribution of Kanie-Tsuchiya, and I just following the intrinsic logic of QFT (which had already led to those 74/75 results), being ignorant about those at that time very recent developments [56].

In those days I had a very intensive collaboration with Rehren. We developed our framework of exchange algebras after making a very careful check in which we had to calculate the positive definite n -point function of the $d = \frac{1}{16}$ field in the conformal Ising field theory [57]. One only had to carry out what Kadanoff and Ceva said (mostly verbally) in their paper and combine this with the simplicity of the “doubled” (Dirac instead of Majorana) model (obviously a positive Wightman theory) by drawing a kind of “holomorphic square root”. Nevertheless the calculations meant hard work, especially for Rehren. Only after this we had the courage to propose the general framework of those new algebras. This care was necessary, the fields were not Wightman fields (this was already mentioned in the previous section) and, in relation to this, the commutation relations were not really group-theoretic-tensorial (but rather the indices were edges on fusion graphs) as one would have expected from a Wightman framework.¹¹ The title “Einstein causality and Artin braids” was chosen in order to indicate the conceptual origin of these new structures already in the title. We were able, among other things, to demonstrate that the general principles of QFT were strong enough

¹¹Tensorial commutation relations are only consistent with abelian braid group commutation relations. In some of the early work on braid group statistics this has been overlooked.

to obtain at least the same explicit insight e.g. into minimal models as the BPZ formalism based on the Virasoro algebra representation theory. We also thought that it should be possible to unravel the rather complicated space-time structure of “plektonic” (i.e. non-abelian braid group representation) correlation functions by “liberating” the Bethe Ansatz from its statistical mechanic compound in order to get a generalized Fock-space description, but this hope (to which I come back later in this section), up to now, did not materialize.

After the arrival of Fredenhagen, who joined us in Berlin (unfortunately only for 3 years), it was possible within a very short time, to describe the exchange algebras within the general DHR framework of superselection-sectors in such a way that finally concepts and formulas hold for all low-dimensional QFT [58].

Parallel to this development, Witten, following the more geometric logic (which also underlies string theory), developed the “topological field theory”. His approach was a quantization scheme where one starts with classical actions and via the mediating power of functional integrals tries to obtain quantum theories. From the classical Chern-Simons action he found, with a lot of hindsight, the Jones polynomial or (in more technical mathematical language) the tracial Markov state on the non-commutative infinite braid group algebra \mathbf{CB}_∞ (he obtained quite a bit more, namely the knot invariants and braids on any Riemann surface and, as some kind of vacuum partition functions, new invariants of general 3-manifolds).

These findings were very surprising to us, since we encountered the Jones invariants in a completely different-looking context, they appeared inside the von Neumann type II_1 intertwiner algebras with their Markov traces as unitary representations of the \mathbf{CB}_∞ braid group algebra (the Markov traces being the ribbon-knot invariants analogous to the \mathbf{CS}_∞ invariants of the DHR methods). The invariants of 3-manifolds which we did not have initially, were obtained later by converting multicoloured cabled Markov traces into Kirby-invariants [FRSII, appendix]. Here we used the idea of some limit endomorphism ρ_{reg} (or rather a limiting tracial state for infinitely thick cables). Wenzl was able to control those limits directly (we only abstracted a formula by using their suggestive power) and in this way characterize the invariants of 3-manifolds as being uniquely related to colour-averaged Markov-states on the pure ribbon braid group \mathbf{RPB}_∞ (the restriction to pure braids is necessary to have a 1-1 relation). With the help of triangulation ideas, we could also obtain the relation to the combinatorial Turaev-Viro theory (with a precise criterion for the conditions under which the Turaev-Viro invariants are the absolute squares of the Witten invariant), but this looked more artificial, at least from a physical viewpoint.

What really surprised us is that Witten could obtain these invariants directly in a seemingly field theoretic way, whereas if one starts from a full field theory with localization and space-time translations one would never be able to see a II_1 von Neumann algebra i.e. a tracial state on \mathbf{CRB}_∞ directly, but only via physical Markov traces on intertwiner subalgebras (here nothing is invented, everything

is derived from physical principles!). How can any quantum theory algebra be directly II_1 (i.e. an algebra which only carries combinatorial data)? In finite-degree quantum mechanics one only sees such structures, if one looks at the Weyl-like algebra going with a particle on a circle (the so-called noncommutative torus algebra, which is not really a Weyl C^* -algebra in the strict mathematical sense).

In order to try to give at least a tentative answer, I have to return to some subtleties in the issue of the Feynman-Kac integral (with an ad hoc chosen action) defining a quantum theory and not every quantum theory being Feynman-Kac representable. The precise conditions have been studied by Klein and Landau [59]. The quantum theory must have the property of “stochastic positivity” (and the stochastic theory must be Osterwalder-Schrader positive). This means that it must admit an abelian subalgebra such that a group of automorphisms (physically: time translations) applied to that subalgebra generates an algebras which is dense in the total quantum algebra. In more physical terms: the theory must be structurally close to a canonical theory of the ϕ^4 -type (the integrals can only be controlled in low dimensions, but for ϕ_4^4 one has at least “structural correctness”).¹²

This is definitely not the case for the Chern-Simons action. Apart from the additional factor i in its euclidean F.-K. representation, it deviates significantly in its canonical structure e.g. in the time-like gauge. The δ -function, which one usually encounters in the commutation relation of the field with its time-derivative now appears between the two spatial components of the gauge field. A simpler example of a theory which violates the Klein-Landau prerequisites, is the one defined by just one chiral current (in this case nobody would dare to write a F.-K. representation).

The K.-L. theorem does of course not rule out the possibility of a Chern-Simons quantum theory (and it does not restrict the use of functional integrals for geometric purposes in mathematics outside local quantum physics). So one could go ahead and try to extract some noncommutative C^* -algebra via a canonical formalism and impose some state on that algebra in order to implement gauge invariance. This was indeed done in the abelian case with the result that the algebra is a kind of Weyl-algebra over 1-forms [60] and the state was that natural singular state (thus implementing the intuitive idea of “summing over all gauge copies”) which already appeared in earlier work. The GNS reconstruction of the Hilbert space representation via that singular state showed the discreteness of type II_1 , i.e. the space-time translation was “killed” in the representation obtained by that singular state, but it is still a shot apart from Witten’s theory.

My suggestion would be to think (as an example) of the Z_N abelian Witten

¹² Structural correctness cannot be checked via quasiclassical approximations. Quasiclassical approximations cannot distinguish local quantum physics from “quantized” infinite dimensional symplectic geometry.

theory as a maximal extension of this 1-forms (as in the simpler case of the Z_N conformal field theory). But one more algebraic step has to be carried out: the globalization of the Z_N Weyl-like algebra \mathcal{A} to a universal algebra \mathcal{A}_{uni} [58] (we will return to this in a different context later). It is hard to say what this means in terms of the semi-infinite string-like 1-forms, without having done the calculations (probably it means that in addition to exact forms one also obtains closed forms in the angular sense). I expect, that a suitably defined gauge invariant state on \mathcal{A}_{uni} will give the Z_N Witten's theory.

If this were true, one would get a rather new view on low-dimensional gauge theories. Whereas in $d = 4$, gauge theory has two aspects, the more physical “quantum Maxwell” aspect and the singular state aspect (“summing over ∞ gauge copies”), only the latter remains in low dimensional gauge theory. Following Narnhofer and Thirring as well as Acerbi, Morchio and Strocchi [61], one should then expect the gauge invariant algebra to be just that subalgebra on which the singular state becomes regular (i.e. the algebraic QFT version of the geometric BRST procedure). For topological field theories this algebra would consist of completely combinatorial “stuff”, whereas if you couple the Chern-Simons gauge field to e.g. spinor matter (extending the Weyl-like algebra in a suitable way with a CAR algebra) then the regular subalgebra would be expected to carry spatial translations and allow localization as usual in QFT.

So, in a novel way, gauge invariant singular states could generate interesting plektonic subalgebras inside old-fashioned bosonic (Weyl-like) or fermionic CAR algebras.

This then would relate to Jones theory (Jones obtains all his subfactors within R , the unique hyperfinite II_1 factor) in a physically deep and perhaps even practically useful way by attributing to low-dimensional gauge theories this very physical role of finding new algebras inside old ones (i.e. something which cannot be done “by hand”), which is the algebraic QFT counterpart of perturbing Hamiltonians.

Having said this, I should add however that I do not negate the fast and efficient power of geometrical methods to produce new and interesting formulas (with later chances of physical interpretation). The most famous historical illustration is that transformation formula which Lorentz and Poincaré shared with Einstein.

It is a bit regrettable that physicist have forgotten the art to obtain new insights by pushing paradoxical-looking situations to their breaking point. The mathematical “fuzziness” of the Feynman-Kac integrals (outside the limitations set by ref.[59]) has propagated into a conceptual fuzziness on the physical side, a kind of geometrical “dreamland”.¹³

¹³Physical words like particles, states, confinement, symmetry breaking, condensation etc. do not mean what they used to mean (and still mean in this article), they rather have become geometrical allegories of local quantum physics.

Almost everything is covered these days by a confidence-creating geometric layer. In earlier times physicists obtained spectacular progress by confronting paradoxical situations (viz. the Bohr atomic model) directly. When I talk these days to mathematicians (with a few significant exceptions) they think we are sharing a happy marriage and live jointly in a beautiful castle, they don't see that they alone live in a castle built on our physics ruins (instantons, . . .).

Most of my physics colleagues agree that some parts of theoretical physics live through a crisis, and some of them even agree that this is home-made (different from the stagnation created by the huge phenomenological success of electro-weak theory).

In a recent panel discussion one could only choose between the scylla of geometrically motivated quantization schemes and the abys of learning how to live happily with cut off dependent real theories like QED, σ -models, ϕ^4 theory etc. The conceptual messages coming from the first half of this century, that one should always aim at theories which are fulfilling all the presently known principles (unless one incorporates them into new ones), seems to have been lost. There is really the danger that this century ends like this, inspite of its conceptually glorious beginning. Even the important message (from Faraday, Maxwell and Einstein) that local structures (action-at-a-neighbourhood principles, locality, causality) are primary and global properties have to be derived from local properties, seemed to be forgotten. Maybe fin de siecle crises are a natural collective phenomenon. In that case one simply would have to wait for another 10-15 years.

The main issue in that panel discussion was the funding of (mathematical) physics, i.e. the economic crisis in physics.

In earlier days, even big physics conferences were profitable, because there were "art critics" who asked many penetrating questions and made interesting and physically relevant remarks. Pauli, Landau, Källén and later also Coleman even shaped the direction of research, although sometimes with misjudgements and prejudices.

I was moved, when Pauli, obviously seriously ill (he died 6 weeks later) after delivering a beautiful lecture on the neutrino and its history at Hamburg University took some rest in "his" turning arm-chair (which we called respectfully the "Pauli chair"). He suffered from fatigue and pains and he said: "ich denke, der Heisenberg liegt mir noch schwer im Magen". He was obviously referring to his involvement in the aborted nonlinear spinor theory which (in contrast to the quark model) was only able to produce particle parities which were "correct up to a sign".¹⁴

Nowadays many talks in planary sessions allow no critical feed-back from an "art critics" physics point of view. They tend to have more similarity with sporting events where highly paid and often anabolically supported athletes give

¹⁴But apart from that the underlying ideas had a certain similarity with quark-model ideas.

their impressive performances. Since my attendance of big conferences was rather limited (the 1994 Paris mathematical physics conference was the only big one I attended during the last 10 years), my impressions may however have no statistical significance. Coleman's yearly "physical weather reports" from Erice still enjoy a prominent place on my shelf.

Since I do not want to end this section on such a pessimistic note, let me report on two interesting recent developments.

The first is contained in a paper by Doplicher, Fredenhagen and Roberts [62] and addresses the question whether a non-commutative version of Minkowski-space is possible (i.e. non-commuting position operators) which leaves the Wigner one-particle structure essentially intact. Well, it is possible, and the physical core of this paper is the derivation of a new uncertainty relation by confronting the quasiclassically interpreted Einstein equation with particle quantum mechanics and invoking a principle of local energy stability against the evaporation into small black holes.

This is very appealing indeed. Fundamental uncertainty relations are hard to get, and the last one after Heisenberg's, was that of Bohr and Rosenfeld for the electromagnetic fields (by consistency of quantum particle theory with Maxwell fields). The saturation of these uncertainty relations by a concrete model algebra is to be understood as a model illustration. In the DFR paper, Einstein causality is replaced by something else, not yet fully understood. Of course one wants to keep macro-causality i.e. causality at large distance. All previous attempts to outmaneuver Einstein causality, e.g. those attempts to admit Lagrangians with formfactors (on a fundamental level), or the Lee-Wick attempt to admit pairs of complex poles in the Feynman rules, failed on macro-causality after building up increasing orders of perturbation theory (leading to non-interpretable precursors). The conceptual "tightness" of Einstein causality lends credit to the belief that any interpretable physically consistent QFT which goes beyond, must necessarily lead towards quantum gravity. As a curious side-result of the DFR investigation one should mention that they also obtain (in their model) a two-sheeted space similar to Connes's results about the non-commutative geometry interpretation of electro-weak interaction phenomenology.

Another interesting proposal is the reconciliation between the renormalization group and the resulting scaling theories with the framework of algebraic QFT, by Buchholz and Verch [63]. I remember my difficulties I had way back, when I wanted to understand the physical content of the Gell-Mann-Low-Stückelberg renormalization group equation; one always seemed to be close to tautologies. After Wilson's and Kadanoff's work and the Callan-Symanzik equation of QFT, this uneasy feeling was removed, but the question remained: what is the intrinsic physical content? The renormalization group calculations played an important role in the construction of field theories starting e.g. from the Feynman-Kac quantization scheme. But suppose one already has a theory, which in the algebraic formulation would be a concrete Haag-Kastler net. Can one construct an

associated scale invariant net? Can the short-distance aspects of quarks (i.e. their role as partons) be understood in that associated theory? Can they be treated (in the scaling theory) as Wigner particles with perhaps new superselection rules (“liberated color”) which in the original theory was not possible? For the answer to some of these questions we refer to the paper quoted above.

4 Modest Aims, What Algebraic QFT Should be Able to Achieve in the Next Future

In QFT, the operator theory of free fields and their Fock-space structure (as unsophisticated they may appear to a young generation raised with differential geometry and algebraic topology) plays a pivotal role historically as well as for its physical interpretation. Most physicist know free fields through one or other form of quantization, few are aware that they are a consequence of purely intrinsic quantum physical principles: if one starts from Wigner’s group theoretic one-particle classification and builds up multiparticle spaces by tensoring (this being the correct formulation for statistical independence in combining subsystems in quantum theory) and invokes the Einstein Causality principle, one invariably comes to this structure. It is also unavoidable if one extracts scattering theory from Haag-Kastler nets. In the latter case one does not only find these free fields as unbounded operators affiliated with the nets, but one also learns that the S-matrix only depends on the nets and not on any particular interpolating field coordinates which one may use as infinitesimal (point-like) generators of the local algebras.

All these are valid statements, if the theory is four-dimensional, in which case the free fields are strictly speaking the old “permutation-group-statistics free fields”. According to the new insights into low-dimensional QFT, this does not happen for $d < 4$. There Wigner particles may obey braid group statistics (for massive two-dimensional theories, the generic situation would be even more “exotic”). In this case the powerful formalism of scattering theory leads to an asymptotic multiparticle momentum space structure on whose unraveling considerable progress has been made recently [64]. But in that case the causality principle (more precisely the “localizability” of fields, since Einstein causality only applies to observables) cannot be simply implemented by Fourier-transformation: “free plektons” would necessarily have a more complicated structure than free bosons or fermions.

Let us look at this situation from a slightly different angle [65]. Suppose we would already know (d arbitrary) an S-matrix with all its creation and annihilation processes. What happens if we consider its “extreme cluster limit” i.e. we spatially remove all particles from each other (in such a way that no pair stays close)? The naive answer that $S \rightarrow 1$ is the correct one in $d = 4$. On the other

hand one would expect that $S_{lim} = 1$ is exhausted by the Borchers class of free fields (although this only has been proven for the case of zero mass particles). So the cluster argument also leads to an associated free field theory.

Let us now try to understand such limiting theories for $d = 2$. Qualitatively one expects a similar behaviour as far as the asymptotic suppression of on shell creation and annihilation is concerned, since the higher thresholds even in $d = 2$ have more regularity properties and since smoothness in p -space amounts to large-distance fall-off properties in x -space. However this simplification cannot go as far as $T = 0$, if T denotes the excess over the identity contribution, i.e. cluster arguments about the S-matrix in $d = 2$ are not able to separate “interaction” from an allegedly non-interacting part (formally: the momentum space δ -functions is the same in front of the two contributions). This is to say that it is difficult to give an intrinsic meaning to the notion of interaction by looking at the two-particle S-matrix (except the statement that the limiting elastic part does not show any higher thresholds). Direct higher elastic processes like $3 \rightarrow 3$ also have stronger x -space fall-off properties as compared to those which happen in subsequent stages through $2 \rightarrow 2$. Hence, if the limiting S-matrix S_{lim} is again a unitary S-matrix in its own right, then the rapidity-dependent $S_{lim}^{(2)}$ has to fulfill the Yang-Baxter equation as a necessary physical consistency equation (i.e. there is nothing to be imposed from the outside). The only surprising aspect is the claim that S_{lim} again belongs to a localizable QFT.

For $d = 3$, an educated guess would be that S_{lim} inherits the energy-momentum independence property from $d = 4$, but it is only locally independent. When two momentum- (or rather velocity-) directions cross, then an Artin R-matrix makes the transition from one momentum space “wedge” to the neighbouring one, similar to the $\theta = 0$ jump for $d = 2$ theories in the Swieca description with S_{phys} . instead of the Yang-Baxter $S_{anal.}$. This presupposes of course that LSZ (or Haag-Ruelle) scattering theory is a good framework for the scattering of “plektons”. (There is a more general formulation which uses only expectation values and not amplitudes).

The picture obtained in this way clearly leads to an identification of “free plektons” (i.e. space-time fields belonging to the limiting theory) and the notion of “integrability” in QFT. It is completely consistent with the results of Coleman and Mandula [66] which were obtained with the help of different ideas.

In fact it seems to be a more profound version of the Coleman-Mandula results, since it leads to a long-distance (in the sense of S_{lim}) universality-class division of low-dimensional QFT’s. Such universality classes of “quasiparticles” (thinking of condensed matter physics where only “localization” is important but not Lorentz covariance of energy-momentum dispersion laws), with precisely one “integrable” (or free-plektonic) representative in each class, are extremely promising concepts with potential experimental ramifications, at least to the extent that “quantum layers” are important for explaining newly observed effects.

Algebraic QFT tells us that the appearance of plektonic statistics and 2-d

kinks is the only significant difference between low- and high-dimensional relativistic QFT. This universality picture would be analogous to the short-distance universality which, for $d = 2$ led to a classification theory of critical indices in the theory of critical behaviour.

Most of my solid state physics colleagues are not aware of the fact that although the physics of critical phenomena is “euclidean”, the classification method which explains the spectrum of critical indices in terms of superselected charges, their chiral fusions rules, and their braid group statistics (and therefore at the end, knot-theoretical invariants) is of course done on the noncommutative QFT side of the mysterious euclidean statistical mechanics versus real-time QFT connection.

Already Kadanoff, to whom some of these concepts were not yet available (and may even have been too exotic in order to be acceptable to him), appealed to the Coulomb-representation although, strictly speaking superselected charges have no direct meaning in the two-dimensional classic stochastic theory (as the value-space of classical field manifolds has no direct meaning in QFT).

A universality-class theory of quasiparticles (with their spin-statistics and charge fusion properties) in the mathematically clear form of free plektons is still a dream about the future, but the analogy with the QFT understanding of 2d critical phenomena gives an impression of how good life could be!

Its origin in relativistic QFT would by no means prevent its use in nonrelativistic condensed matter physics inasmuch as the relativistic proof of the spin-statistics theorem and the notion of antiparticles does not foreclose their validity in the nonrelativistic physics of fermions and bosons.

It is however not so easy to see whether ad hoc introduced degrees of freedom like “spinons” and “holons” could naturally result from such a more general looking scheme, which is still severely restricted by “localizability”. The spin and statistics of nonrelativistic plektons is more flexible than that of bosons and fermions, but there still will be a spin-statistics connection!

There is also the equally important question to what extent physical phenomena like superconductivity (old and new) are really universal. Is it really true that the relation between T_c and the properly defined gap in a theory with only 4-fermion interactions (the original BCS model) does not depend on the details of the coupling (whereas T_c and Δ do depend) and can only be changed by changing the degrees of freedom of the system (e.g. by introducing phonon degree’s of freedom)?

As a non-expert I would interpret the general acceptance of results of approximation-methods like Hartree-Fock, random phase etc. as an indication of universality of that relation. But then, at least in pure quadrilinear fermionic theories, one could only change that relation by giving those fermions an internal, say SU(2) degree of freedom by which the relation would get immediatly modified by a factor 2^2 . This is of course physical nonsense since nobody can imagine a phase transition which converts ordinary U(1)-symmetric condensed matter into SU(2) matter. But plektons are different, they are as selfdual as a U(1) theory (or

rather Z_N -theory if one quantizes the charges), even though they are more non-commutative! In fact phase transitions away from the perturbative fermi-liquid phase provide the only conceivable mechanism by which they can appear.

If this is so, then the amplification factors in the T_c/gap relation should be interpretable in terms of squares of statistical dimensions, which is the same as Jones indices!

In the literature one finds many geometric investigations of braid group statistics based on the Aharonov-Bohm or the Chern-Simons theory, see for example the work of Wilczek [67]. Since I have already commented on the difficulties in relating Chern-Simons with quantum physics, let me now make some remarks on the use of the Aharonov-Bohm effect.

The physically measurable A-B phase shift of this effect is completely independent of the subtleties one encounters if one uses this long range A-B interaction for pair-interactions between particles. Whereas in the first case, only quasiclassical properties are relevant, the boundary condition which one should use in the scattering theory of such long-range interactions are not so clear. If one wants to use the A-B theory for the description of particle statistics and is inclined to take some hints from algebraic QFT, then it should be chosen in such a way that the cluster property holds, i.e. the three-particle A-B theory should contain in it the say previously understood, two-particle theory and so on, like an infinite russian matrushka. The correct boundary conditions should be those which are consistent with these cluster-properties which would convert the original two-particle problem into one with infinite degrees of freedom.

Of course I do not know whether such a “bootstrap” version of the A-B theory is feasible, the only thing I am saying is that it ought to, if this effect can be used to describe abelian braid group statistics i.e. “anyons”.

I am convinced that braid group statistics in Q.M. cannot be analyzed as an issue separated from QFT. In the nonabelian case the above “cluster tower” is reminiscent of the algebraic Jones tower in the subfactor theory. Using this analogy, the cluster properties of nonabelian A-B theories should lead to a quantization similar to Jones or to that of statistical dimension as in algebraic QFT. In QFT these cluster or tower (tunnel) aspects are built in through the net structure, and the localization principle for the physically admissible state on such algebras, unlike Q.M., they do not have to be added “by hand”.

Pure geometric considerations alone, whether in the original form of Leinaas and Myrheim, [68] or in the mathematically refined recent version by Mund and Schrader, [68] constitute the geometric prerequisites, but for themselves are not sufficient in order to obtain a plektonic quantum mechanics.

The issue of “new degrees of freedom” has also progressed significantly in lattice models [69] of statistical mechanics. There, the “old degrees of freedom” which correspond to fermions and bosons of continuous QFT, are “Paulions” i.e. lattice spins repeated on all sites.

In the language of quantum spin chains one obtains an infinite spin chain

as a limit (using appropriate boundary conditions) from finite chains. In the case of free boundary conditions this corresponds precisely to the constructions of a so-called UHF algebra $\lim_{n \rightarrow \infty} \text{Mat}_2(\mathbf{C})^{\otimes n}$ from finite full matrix algebras, with the lattice labels being the floors of the Bratteli tower (this only gives a semiinfinite lattices, the full lattice can be obtained afterwards by translation). The corresponding Bratteli diagrams for Paulions are of the most trivial type: they have a period two, and in case of $\text{Mat}_2(\mathbf{C})$ also width two.

In a profound paper Pasquier [70] realized that (using Ocneanu ideas) one gets a tremendous richness if one allows a width which successively increases up to a maximal value (related to the depth of Jones inclusions). In this way he was able to describe the kinematical aspects of local observable algebras underlying the so called RSOS models of Andrews, Baxter and Forrester.

Recently this idea was extended by Jones (with the help of “commuting squares”, a kind of “pre-braid” formalism) in order to incorporate the three physically important families into his subfactor framework: spin models,¹⁵ vertex (=SOS) models and (for the new degrees of freedom) RSOS and IRF models (the latter making their appearance through Hadamard matrices in the commuting square scheme). In addition, he was able to obtain a subfactor interpretation of periodic boundary conditions which have thermodynamic limits outside the above AFD algebras (i.e. those which one can obtain by Bratteli diagrams and inductive limits) and lead to “affine” Hecke algebras [71]. All these new degrees of freedom permit the construction of local lattice theories (like RSOS) via Hamiltonians or transfer matrices. Although the number of possibilities is “myriotic”, one expects to recover (through scaling limits) only those continuous new degrees of freedom which are permitted by the principles of algebraic QFT (in case the lattice models have second order phase-transition points).

One would hope that algebraic QFT is capable of combining all those different discoveries, including those in conformal field theory (independent of whether they had been made by “truffle hogs” or supersophisticated mathematical physicists) under one roof: the classification theory of low-dimensional QFT’s and the explicit construction of their free plektonic representations. Despite some nice progress in the understanding of momentum space behaviour of plektons through the powerful formalism of scattering theory, I think that there is one important corner stone missing: quantum symmetry.

Internal symmetry in ordinary quantum physics amounts to representation theory of compact Lie groups. This is at least our picture ever since Heisenberg introduced isospin. It took a very long time and, physically as well as mathematically, sophisticated work, to see that group symmetry is a consequence of quantum field theoretical permutation group statistics which in turn (at least for $d = 4$) results from Einstein causality of observables and localization of states.

¹⁵ The inclusion related to the above Paulion chain is that of Z-components of spin (diagonal matrices) into the full matrix algebra.

The DHR [1] work and its DR [72] completion, despite its monumental conceptual enrichment, has not found widespread appreciation, and the reason for this is obvious. They proved that what one can derive from intrinsic quantum principles is just the same as what one had known all along from quantization of classical Lagrangians, at least as far as internal symmetry in $d = 4$ is concerned.

There exists a more nonlocal looking but physically equivalent description of QFT where the charge-carrying fields are not multicomponent bosons or fermions but rather “parafermions”¹⁶ of some finite order equal to the height of the statistics Young tableaux which obey R-matrix commutation relations with trivial monodromy $R^2 = 1$ [73]. They live in a smaller Hilbert space without the group theoretic multiplicities. They lead to the same scattering observables and are built on the same observable-algebra, but the amplification factors in cross sections (which in the group theoretic description enter through traces over Casimir-operators) originate for parafermions from the unusual inner products for scattering states (given by a Markov trace on the permutation group). The standard description in terms of multicomponent fermions and bosons is the better one: it is the only one which allows a Lagrangian formulation i.e. a description by quantization, whereas the para-statistics-description is too non-commutative for that purpose. Physically more important is that the standard description is required if it comes to the important issue of symmetry-breaking (an issue which has nothing to do with quantization).

Around 87/88 the question of what symmetry concept is behind braid group statistics arose, and it was clear that it cannot be a symmetry arising from a compact group.

There was an obvious formal relation between the “ q -dimensions” of quantum groups and the DHR-Jones-Wenzl quantization via tracial Markov states on the Jones-Temperley-Lieb algebras or on CB_∞ braid group algebras respectively (in the original DHR approach on CS_∞). So it was very tempting indeed to interpret quantum groups as being the (dual) symmetry behind braid group statistics. In a spirit of excitement I was already on my way (in 87/88) to write up notes, when Rehren (to whom I am very grateful) pointed out some flaws of quantum groups in relation to the superselection theory. Some of these (the more mathematical ones) were later overcome in the work of Mack and Schomerus [74] by the introduction of “weak quasi”-Hopf algebras. But the nonuniqueness of their construction (in the DHR theory the symmetry was unique) as well as the somewhat asymmetric looking treatment of antiparticles (i.e. the conjugation in weak quasi-Hopf algebras) dampened my enthusiasm somewhat. The asymmetry property is of course shared with our previous “exchange algebras”, but they at least were unique. Their algebraic relations (as well as ours) are incomplete i.e. a distinction between non-overlapping and overlapping localization has to be made.

So I began to ask the question: why do the well-understood unique exchange

¹⁶We use the word in the Green, Messiah, Greenberg, and DHR sense, but not in Kadanoff’s.

algebras in the DHR framework (where they amount to parastatistics with R-matrix commutation relations) are less than acceptable as compared to the DR compact Lie group (i.e. the standard) tensorial description?

One answer was a very pragmatic one. Old theorems like the previously mentioned Jost-Schroer-Pohlmeyer [2] theorem which relates e.g. the algebraic assumption of free field equations (as defining an ideal within a formal field algebra, the Borchers algebra) to the free n -point correlation function with its Wick-combinatorics, such old theorems run into trouble with parastatistics fields. In that case one does not start with tensorial fermions or bosons (the para-fields are localizable i.e. have R-matrix commutation relations, but they are not local) and one is not supposed to end with the Wick-combinatorics either: parastatistics free fields have a much more complicated combinatorics. All known formalism, including the nonrelativistic generalized Hartree-Fock methods, rely on local tensorial fields. In addition there is the important issue of symmetry breaking and one does not have the slightest idea of how to do this with Mack-Schomerus fields.

In this complex situation I went back and looked again at our previous construction of the universal observable algebra \mathcal{A}_{uni} which we elaborated in algebraic conformal QFT (following a previous more abstract proposition by Fredenhagen). The physical idea which led to its construction was very simple. Whereas in the old DHR theory with permutation group statistics, the net was naturally directed towards Minkowski-space infinity and therefore you could construct outer endomorphisms (charged sectors) as a limit of inner charge transport by just disposing any unwanted anti-charge into the big waste basket at infinity [1], this was not possible for non-directed nets as e.g. conformal nets.

In that case, the only natural globalization was that through free, amalgamated (over all local relations) C^* -algebras, namely \mathcal{A}_{uni} . The key physical idea was that every localized endomorphism ρ (or its sector-equivalence class) gives rise to a global selfintertwiner [FRSII, ref.58] V_ρ (charge-transport “once-around”) described by a unitary operator within \mathcal{A}_{uni} , and that its use in an intertwining chain:

$$\text{vacuum} \xrightarrow{\text{split}} \text{charge-anticharge} \xrightarrow[\text{of charge}]{\text{selfintertwining}} \text{charge-anticharge} \xrightarrow{\text{fusion}} \text{vacuum}$$

leads to invariant global charges and (by spectral decomposition) to invariant projectors inside \mathcal{A}_{uni} , which project onto the various central components of \mathcal{A}_{uni} . It immediately led to a very profound understanding of Verlinde’s observations on characters of Virasoro or W-algebras and in addition one obtained that the globalized exchange algebra is really living on a helix (the covering of S^1): the true braid group relations are those of B_∞ on a cylinder and not on the plane (or on a Riemann sphere). The covering is finite (a certain power of the self-intertwiner is trivial) if the model is “rational”, but otherwise our results were of a completely general model-independent nature (it can be easily transferred to 3-d theories).

Recently I realized that there is much more in \mathcal{A}_{uni} . If one uses these inter-

twiners V_α inside more complicated splitting chains according to:

$$\rho \xrightarrow{\text{split}} \alpha \circ \beta \xrightarrow[\text{selfintertwining}]{\alpha \rightarrow} \alpha \circ \beta \xrightarrow{\text{fusion}} \rho'$$

(where α and β are two irreducible intermediate sectors)

then the result is that within \mathcal{A}_{uni} one can construct intertwiners between sectors ρ and ρ' (which only depend on the α, β equivalence classes). In such way one easily obtains charge-creating intertwiners which e.g. connect the vacuum with some other irreducible sector. There is, however, a glitch to this: the intertwining is a C^* -algebra property which is lost in its vacuum representation. It only becomes activated in higher representations i.e. in the presence of other charges. For this reason I called this mechanism “charge-polarization” symmetry [75]. According to my best knowledge it is also a mathematically new phenomenon, i.e. not part of known representation theory. It can be traced back to the “freeness” aspect of the \mathcal{A}_{uni} C^* -algebra. For “rational” theories (i.e. a finite number of sectors) one can see that the subalgebra of such intertwiners which one obtains by fixing a basis of endomorphisms is finitely generated with maximally N^3 generators in the case of N sectors; the third power is coming about because for each sector there are N global V_ρ^n -intertwiners). This algebra leads to a refinement of central projectors to non-central ones: just multiply the central projectors with the various projectors obtained by spectral decomposition of V_ρ^n , using the finite spectrum (monodromy phases) which those operators have on the generating fusion intertwiners.

For the first time, one found a structure which is characteristic for the physics of plektons, it is blind against fermions and bosons. In the DHR theory, global charge operators can only be found in specific representations (as weak limits) and never in the global so-called “quasilocal” observable algebra with that big waste disposal at infinity.

If you ask what geometric structure this global intertwiners algebra represents, my tentative answer is that the polarization symmetry has a very intimate connection to some sort of “universal mapping class group”-algebra amalgamated over “something”. Although I have no proof for this (this notion is presently not even well-defined), I have done numerous graphical checks using the physicist’s results on mapping class group representations in conformal field theory or combinatorics.

I am convinced that the attempts to define and classify conformal QFT in terms of objects on Riemann surfaces is like doing things upside-down (at least from a quantum physical standpoint).

There are two ways in which Riemann surfaces may be related to chiral conformal QFT. On the one hand they may serve as a mnemotechnical device to keep track of the ternary fusion structure and the combinatorics. On the other hand, Riemann surfaces could also serve as the generalized Bargman-Hall-Wightman domains of analytically continued correlation functions (but never as the local-

ization space on which fields “live”) which then would have complicated automorphic properties. These complicated automorphic properties could result by averaging the original real-time correlation function with the help of Fuchsian groups (one formally maintains the positivity on light rays). Whereas for $g = 1$ this averaging can be controlled [76] and dumped into the states (which thereby become the well-known L_0 -temperature states), there is no understanding of these formal manipulations for $g \geq 2$. In this case, also the algebra seem to suffer a radical change, and it is unknown, what this means in terms of algebras and states.

On a very formal level one makes, however, the curious observation that the original net indexing (in this case by intervals on the light ray) loses its meaning in terms of Einstein causality and one finds a kind of super Reeh-Schlieder situation: what used to be the old net members now become dense in the total algebra (as a result of the Fuchsian ¹⁷ averaging). This is one possible scenario of how Einstein causality may be outmaneuvered: there is no global notion of space-like and hence no necessity to find compatible (commuting) measurements in causally disjoint regions. The standard causality could then only survive in the infinitesimal.

Similarly the 3-mf. invariants cannot be related to the “living space” of fields (TFT cannot be used to extract information on localization), they rather belong to that ill-understood “twilight zone” of external/internal symmetry.

In discussing the aforementioned global structures of \mathcal{A}_{uni} with Karowski, I also became aware of the fact that the selfintertwiners V_ρ correspond to lines around handles in their [77] (together with Schrader) formulation of invariants of graphs on surfaces. This gives a geometrical interpretation of the strange “flip relations” which these global intertwiners have with local ones. I also learned from him that by extending somewhat the Pasquier lattice construction of generalized ABF-like models, one may be able to see some of the above charge creating properties already on a finite lattice [78].

The crucial test for physical usefulness of quantum symmetry based on the charge polarization idea, however, has not yet been carried out. In analogy with the parafermions versus compact Lie group fermions of before, I think that it should play a crucial role in converting the momentum-space description of plektions into localizable free plektonic x -space fields with a “generalized Fock space”.

The relation of nonlocal fields to localizable fields has an interesting analogue in the Bethe-Ansatz approach to integrable lattice models. There the so-called B-field, which related to the pseudo-vacuum and the generated pseudo-excitations, gives rise to the physical vacuum and localizable physical vector states whose relation to localizable charge carrying fields is, however, technically too difficult

¹⁷The Fuchsian groups for higher genus are discrete subgroups of the symmetry group of the vacuum state.

in order to be explored analytically (a typical difficulty of lattice physics, where the start is always conceptually easy, but life becomes very tough later on).

So what I am presently “dreaming” about is that QFT “liberation” of the Bethe-Ansatz idea, which Rehren and I verbally already mentioned in our “Einstein causality and Artin braid” paper. In any case, I do not share Fredenhagen’s optimism (expressed at the Paris satellite conference) of expecting to get the full description of the x -space localized plektons including their n -point correlation functions within the extended exchange algebra framework (without using “quantum symmetry”).

There is another potential use of these ideas, this time of a more mathematical nature. The present understanding of invariants of 3-manifolds either by functional integral or combinatorial recipes or even (via the ρ_{reg} -idea mentioned before) with algebraic QFT leaves a lot to be desired from a physical-philosophical standpoint.

Analogous to the Coleman-Mandula-O’Raifeartaigh [66] No-Go theorem concerning the marriage between space-time symmetries and inner symmetries in the old theory, there should be a compelling reason, why now, in the context of braid-group statistics, these two concepts are inexorably linked. This is of course an issue which was already there ever since Jones derived knot invariants.

Algebraic QFT and the Jones subfactor theory gives a very good mathematical control and describes this phenomenon very well, but, at least presently, it does not really “explain” it.

One would hope, that a quantum symmetry concept, which is sufficiently different from the old one, is able to achieve that. It is interesting that Popa, [79] in some recent work, expresses the hope that some “free product” construction for operator algebras may help in unravelling some yet hidden geometrical structures in the Jones theory. I think that algebraic QFT has enough conceptual richness to solve such problems and I hope that in this way we may turn around the (up to now) rather one-sided subfactor – algebraic QFT connection.

A real high-energy particle physicist, in taking notice of these developments, may think that these are isolated ideas, perhaps applicable to condensed matter physics, but never of any relevance to 4-dimensional gauge theory and quark physics. I do not share such a pessimistic view.

On the contrary, I think that after a much better understanding of low-dimensional QFT has been achieved, mathematical physicists will revisit gauge theory and the not yet understood problem of quark confinement, and finally realize that the relation of e.g. QCD to a new theory (not built on quantization) will be similar to the relation of the quasiclassical Bohr atomic model to the full Q.M.. Comparing the successful low-dimensional concepts with the gauge theory based on the quantization of fibre bundles, one gets the impression that QCD is much too classical for a fundamental quantum theory.

Why (apart from an associated short distance scaling theory as mentioned before) should quarks in their role as physical spectrum generating objects be

describable as Wigner particles (i.e. have a welldefined spin and mass assignment as it is characteristic for the class of localizable finite energy states)? In what sense can QCD lead to the problematization of the concept of “magnetic field” with an ensuing “self duality”, generalizing that of plektons (on the same profound quantum level as the recent insight into low-dimensional QFT arose from a problematization of “charges”)?

I would like to think of quarks as being basically physical quantum states¹⁸ whose main “unphysical” property is that they have infinite energy (and very weak localization properties). According to our present best knowledge, semiinfinite space like cone localization either corresponds to spectral mass gaps (in that case there is unitary equivalence to arbitrarily thin i.e. Mandelstam string-like localization) or to infraparticles (where the shape of the photon cloud determines the localization size).

Therefore infinite energy quantum states, which are even more remote from Wigner particles than infraparticles, are likely to admit a localization which is not better than that around space-like surfaces. This then could again lead to a rich geometric structure (space-like surfaces allow for a generalized braiding) and a possible generalization of statistics.

Zamolodchikov’s invention of the tetrahedral equations (with promising attempts of Baxter et al. in the direction of 3-dimensional integrable lattice-model building), together with the necessity to find a quantum problematization of the classical notion of “magnetic field” (as it was successfully accomplished for “charges”) are ominous and hopeful signs. But presently one lacks a physical principle leading to such strange states and fields.

After the discovery of Q.M., almost no conceptual breakthrough has been made with just one attempt. A good illustration is conformal QFT, which required three attempts (well separated in time) in order to reach its present perfection. One would be inclined to believe that deep notions on the QFT S-matrix connections (like e.g. the Borchers equivalence classes of local fields) will have their comeback on a higher level of conceptual sophistication. Since the present generation of theoretical physicists has a very superficial knowledge of relativistic scattering theory (some none at all, or only through the string theoretic caricature of an S-matrix) this will probably not happen in the foreseeable future, unless the older generation does transmit the knowledge of these incomplete old discoveries through lectures and in courses on QFT. If, however, physics loses its historical connection and in every decade new ahistorical inventions with new terminologies are proposed, or if mathematical physics is just used for spare parts in pure mathematics, then our future is rather doubtful. In that case I also do not understand why society should financially support our activities.

¹⁸I do not think that Lagrangian gauge theories like QCD would allow to do that (see chapter 2, page 15). For critical observations on the quantization of nonabelian gauge theories by functional integrals, see [80].

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The chapter I-XII are a useful source of information on the “first generation” work on two-dimensional solvable models.
All the calculations in the book which illustrate “screening versus confinement” were done with the correct picture in mind, (Confinement = surpression of fractional flavour which one adds to the model as a testing charge) as in the Kurak-Schroer-Swieca paper quoted therein.
Nevertheless at the end of the conclusions in 10.4 the old aborted ideas of

linking this distinction with poles (and scattering states) again are looming through. To be sure, one can discuss the issue of “free charge versus screening versus confinement” concretely with correlation functions, but than one has to use the criteria given in:

K. Fredenhagen, “Particle Structure of Gauge Theories”, published in Erice school on Mathematical Physics 1985, p.265.

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The conformal QFT in most of the post 1980 papers is presented in the following form: one contemplates the existence of a chiral field $\varphi(z)$ with z not just on the circle or the real line but being a variable in the complex plane and imagines holomorphic transformation properties in that variable.

It is well-known that this is not even true for free fields (the positive frequency part can be extended to the upper half-plane and the negative frequency part is analytic in the lower half-plane). Of course there are vector-states and correlation function (but not algebras and operators!) which have analytic properties, namely those studied by Bargman, Hall and Wightman [2]. But the global conformal transformation properties in the z -variable of analytically extended correlation functions in most of the literature are incorrect: they simply contradict the subtle analytic branching properties of those functions (e.g. the relevance of $SL(\widetilde{2}, R)_c$ versus $SL(2, \mathbb{C})$).

Fortunately people use calculational recipes which allow to bypass the confrontation of what they say with what they think they mean. But in conceptual or structural problems as those of the aborted paper [4] or the resolution of the Einstein causality paradoxon via central projection of the present paper, this “mild” muddyness would lead to disaster.

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According to my best knowledge this is the most poignant criticism within the standard setting of QFT (e.i. not using methods of algebraic QFT).